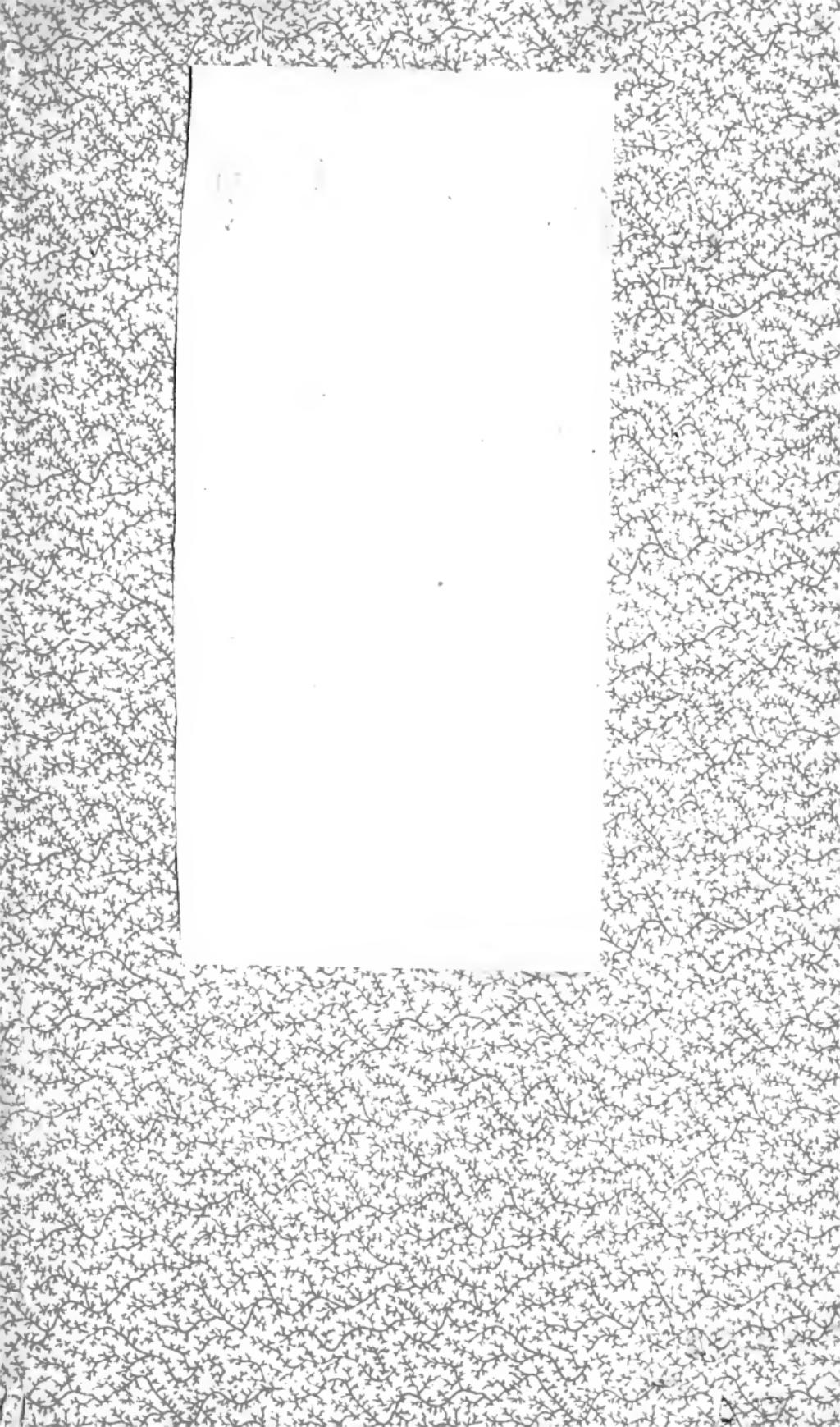
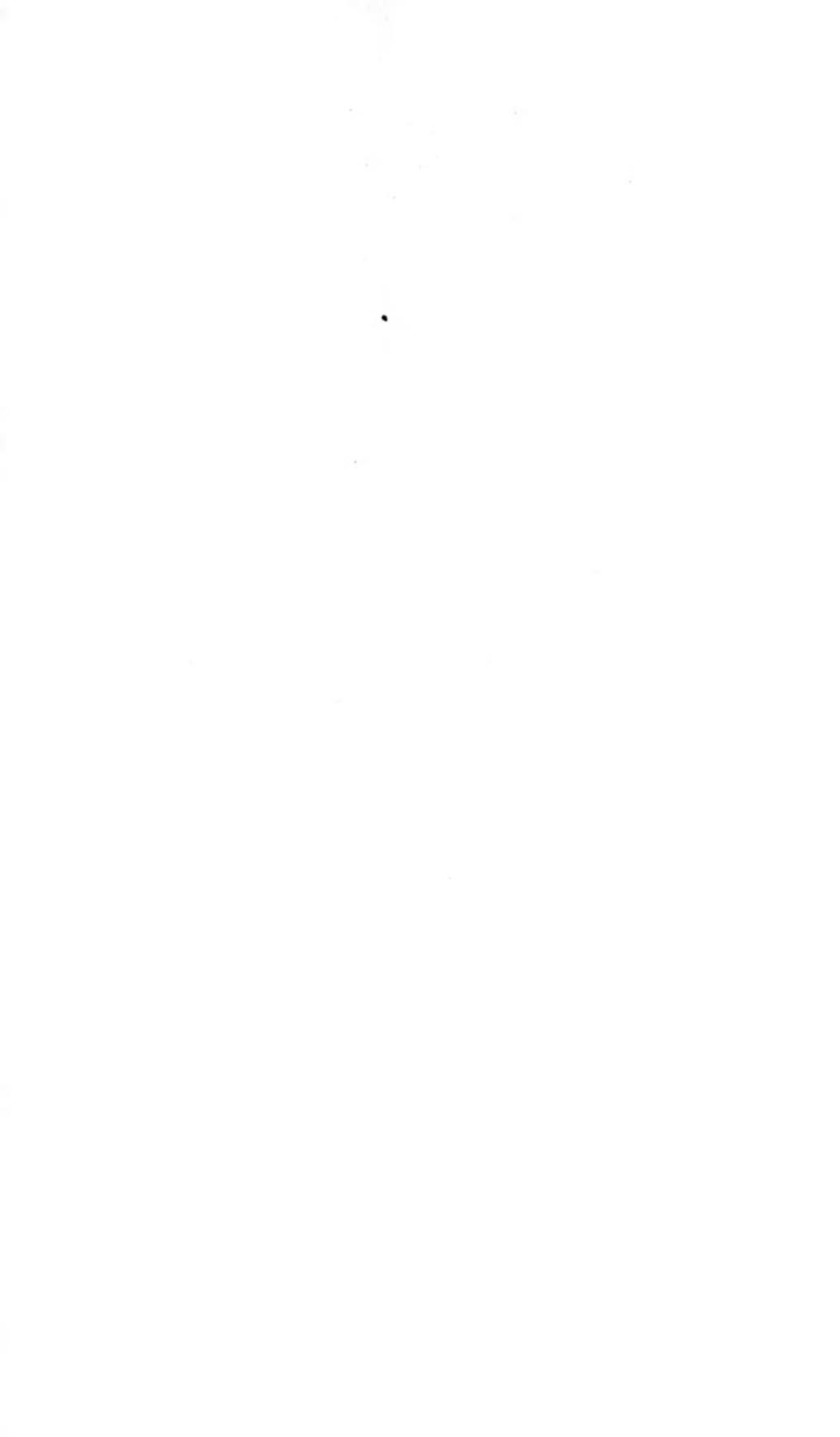


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LETTER

TO

HENRY LORD BROUGHAM, F.R.S. &c.,

CONTAINING

REMARKS ON CERTAIN STATEMENTS

IN HIS LIVES

OF

BLACK, WATT AND CAVENDISH.

BY THE

REV. WILLIAM VERNON HAROURT,

F.R.S. &c.

WITH AN APPENDIX,

CONTAINING

NEWTON'S LETTERS ON AIR AND AETHER.

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From the LONDON, EDINBURGH, and DUBLIN  
PHILOSOPHICAL MAGAZINE AND JOURNAL OF SCIENCE.

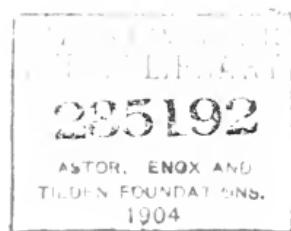
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LONDON:

RICHARD AND JOHN EDWARD TAYLOR,

RED LION COURT, FLEET STREET.

1846.



# LETTER

TO

HENRY LORD BROUGHAM, F.R.S., &c.,

CONTAINING

REMARKS ON CERTAIN STATEMENTS IN HIS LIVES OF

BLACK, WATT AND CAVENDISH.

MY DEAR LORD,

IN a volume of biography which you have lately published, I perceive that you have reprinted your contribution to M. Arago's historical notice of Watt, in which the distinguished author attempted to transfer to the subject of his *éloge* the credit of a celebrated chemical discovery, hitherto by the common consent of chemists attributed to Cavendish.

Your personal challenge to myself would not have moved me to enter again on a question which I scarcely think open to dispute since the fac-similes of Cavendish's original notes of that discovery were printed in the Transactions of the British Association for the Advancement of Science, had I not observed, as it seems to me, other mistakes in this volume on points of scientific history, which, venial as they are in one who cannot be supposed to have devoted much of his valuable time to these *umbratile studies*, are yet such as ought not to pass without some notice.

I must begin, however, my criticisms on your historical chemistry, by repeating the grounds on which I deemed it needful to controvert the statements of M. Arago respecting

the discovery of the composition of water. "The *éloge* of Watt, delivered before the French Academy by one of its secretaries, and subjoined to the *Annuaire* for 1839, had just been published. It was blemished by statements which reflect unjustly on the character of one whose memory is cherished among us as a bright example of the union of modesty with science, of the purest love of truth with the highest faculties for its discovery, and the most eminent success in its attainment. Perceiving these statements to be founded in error, I took the earliest opportunity of rectifying them, at the meeting of the British Association which followed within two or three weeks after I became acquainted with them, rejoiced that I had it in my power, from the position in which, as President of that body, I had then the honour to be placed, to make the correction of the error as formal and public as its promulgation had been; and persuaded that M. Arago, as soon as he should be fully possessed of the facts, would consider it a duty which he owes both to the Academy and himself, to retract the suspicions which he had expressed\*."

Those who feel that a sense of justice is a material part of the character of illustrious men and illustrious bodies, are still "waiting," not "till your fellow champion," as you express it, "shall seal your adversary's doom," but till he shall make the *amende honorable* by withdrawing, in explicit terms, imputations which since the lithographing of the Cavendish MSS. he must know to be unfounded. I am not content, my dear Lord, that you should either for your "colleague" or yourself, half retract and half retain those doubts which I perceive that you have *republished* in one part of your volume, whilst you *disclaim* them in another.

"I cannot easily suppose," you say, "that M. Arago ever intended, and I know that I never myself intended, to insinuate in the slightest degree a suspicion of Mr. Cavendish having borrowed from Mr. Watt." Certainly, as regards yourself at least, no declaration can be more explicit than this. But what then, give me leave to ask, is the significance of the following words in your *now republished* appendix to M. Arago's *éloge*? "Whether or not Mr. Cavendish had heard of Mr.

\* Report of the British Association for 1839, p. 22.

*Watt's theory previous to drawing his conclusions, appears more doubtful: the supposition that he had so heard rests on the improbability of Sir C. Blagden and many others knowing what Mr. Watt had done and not communicating it to Mr. Cavendish, and on the omission of any assertion in Mr. Cavendish's paper, even in the part written by Sir C. Blagden with the view of claiming priority as against M. Lavoisier, that Mr. Cavendish had drawn his conclusion before April 1783. Mr. Watt's theory was well known among the members of the Society some months before Mr. Cavendish's statement appears to have been reduced into writing, and eight months before it was presented to the Society. That the first letter of April 1783 was for some time,—two months as appears from the papers of Mr. Watt,—in the hands of Sir Joseph Banks and other members of the Society during the preceding spring, is certain from the statements in the note to p. 330; and that Sir C. Blagden, the Secretary, should not have seen it seems impossible, for Sir Joseph Banks must have delivered it to him at the time when it was intended to be read at one of the Society's meetings (Phil. Trans. p. 330, note); and as the letter itself remains among the Society's records in the same volume with the paper into which the greater part of it was introduced, it must have been in the custody of Sir C. Blagden. It is equally difficult to suppose that the person who wrote the remarkable passage already referred to respecting Mr. Cavendish's conclusions having been communicated to M. Lavoisier, should not have mentioned to Mr. Cavendish that Mr. Watt had drawn the same conclusion in the spring of 1783, that is, in April at the latest; for the conclusions are identical, with the single difference that Mr. Cavendish calls dephlogisticated air water deprived of its phlogiston, and Mr. Watt says that water is composed of dephlogisticated air and phlogiston.”—(Life of Watt, pp. 396–398.)*

To what does all this argument tend?—Would it lead any one to guess that you mean to acquit Cavendish of plagiarism, or that “you have yourself,” as you elsewhere affirm, “always been convinced that Mr. Watt had, *unknown to Cavendish*, anticipated his great discovery?” Allowing a certain interval

of time and place, I should not wonder at your having forgotten or laid aside your doubts whether Cavendish, with the connivance of Blagden, had not purloined the conclusions of Watt; but I have never before known an instance of so deliberate a disavowal of a suspicion contemporaneous and in juxtaposition with its no less deliberate reiteration.

Your *reprinting now* these old doubts is the more unaccountable, not only because they consist so ill with your profession of belief in the good faith of Cavendish, and are indeed a mere trifling after that point has been satisfactorily established, but because I have corrected the *particular* error out of which this tissue of suspicions was spun; and you are now apprised that the Secretary of the Royal Society at that time was *Mr. Maty*, and not, as you persist in taking for granted, Cavendish's friend *Dr. Blagden*, who did not enter on the office till *May 1784*. "So that," as I told you in the Postscript to my address to the British Association, "he is not liable to the suspicion intimated by Lord Brougham, of having shown Watt's letter to Cavendish, nor to the reproach which M. Arago casts upon him, of not speaking the whole truth respecting the precise date at which Watt's opinions were made known in London."

The confidence which you place, with so much simplicity, in the innocence of M. Arago's "*intentions*," contrasts strangely with the disposition you have shown to suspect Cavendish and Blagden: for M. Arago does not, like yourself, "just hint a fault," but retorts in good set terms on the English philosophers the imputation which Blagden had cast on Lavoisier, "That he had told the truth, but not the whole truth." "This is a heavy charge," says your illustrious colleague; "let us see whether *all who took part in this affair* are not liable to the same reproach\*;"— and then in a style of pointed irony, into the spirit of which I should have thought you apt enough to enter, he proceeds to fix the charge on Cavendish and his

\* " *Lavoisier, ajoute enfin Blagden, a dit la vérité, mais pas toute la vérité, un pareil reproche est sévère. Fut il fondé, n'en atténueraï-je pas beaucoup la gravité, si je montre que, Watt, excepté, tous dont les noms figurent dans cette histoire s'y étaient plus ou moins exposés.*"—*Annuaire, 1839*, p. 333.

fellow-conspirators. I believe I have given no more than the plain meaning of these clever sarcasms when I said, "The Secretary of the Academy has not confined himself to taking from Cavendish the honour of this discovery, but has cast a cloud of suspicion on his veracity and good faith; he has, in fact imputed to him the claiming a discovery which he borrowed from another; of inducing the Secretary of the Royal Society to aid in the fraud, and even causing the very Printers of the Transactions to antedate the presentation copies of his paper\*."

The real truth is, that M. Arago having, when in England, heard but one side of the story, was persuaded of the *insincerity* of Cavendish. If he is now disabused of this persuasion, I hope he will choose another method of withdrawing what he wrote under such an impression than that which you have framed for him in the following protest. "As a strange notion seems to pervade this paper that every thing depends on the character of Cavendish, it may be as well to repeat the following disclaimer, already very distinctly made, of all intention to cast the slightest doubt upon that great man's perfect good faith in the whole affair, I never having supposed that he borrowed from Mr. Watt, though M. Arago, Professor Robison and Sir H. Davy, as well as myself, have always thought that Mr. Watt had, unknown to him, anticipated his great discovery."

Of the deceased philosophers, whose names are here pressed into this service, I shall presently have occasion to speak; but let me first venture to answer for M. Arago, that if *he* has "read the fac-similes" of Cavendish's notes, you will not find him at the same loss as yourself to discover the *inferences* of the experimental philosopher in *the steps of his investigation*;

\* "Pour rendre l'imbroglio complet, les protes, les compositeurs, les imprimeurs des *Transactions Philosophiques* se mirent aussi de la partie. Plusieurs dates y sont inexactement rapportées. Sur les exemplaires séparés de son mémoire que Cavendish distribua à divers savants, j'apprécie une erreur d'une année entière. Par une triste fatalité, car c'est un malheur réel de donner lieu involontairement à des soupçons facheux et immérités, aucune de ces fautes d'impression n'était favorable à Watt!" — *Annuaire*, 1839, p. 335.

he will not join you in propounding, “that in all Cavendish’s diaries and notes of his experiments, not an intimation occurs of the composition of water having been *inferred* by him earlier than Mr. Watt’s paper of spring 1783.”

Those celebrated experiments of 1781, which pass with chemists for a model of a well-combined train of analytical and synthetical research, you imagine to have been without object or *inference*, till an erroneous attempt to repeat them had the good luck to be reasoned upon by Watt in 1783. You appear to think that the manner in which the great discoveries of experimental philosophers are made is by one man’s stumbling on the *proofs*, and another some time after hitting on the *conclusion*. If it be so, I believe that you would have been as capable of interpreting such experiments, once made, as James Watt himself; and could you have been at hand when Cavendish, in July 1781, ascertained that the water, into which he had found two volumes of hydrogen and one of oxygen to be convertible, is pure, he need not have waited so long to learn what to *infer* from this fact: I doubt not but that you would at once have drawn the *inference* for him, established the *theory*, and in your own amended phrase, “*unknown to him, have anticipated his great discovery.*”

But I own I do not suspect your “colleague” of these peculiar views. Once satisfied that Cavendish spoke truth when he said that all the experiments on this subject published in his paper were made by him in the summer of 1781, he will no longer doubt to whom the discovery in question is due; once convinced that the experiments were communicated to Priestley, and that the attempt to repeat them was made in consequence of that communication; once aware that the repetition was abortive because made with a *wrong* gas, that neither the phlogiston nor the inflammable air of Priestley and Watt were convertible terms for *hydrogen*, and that their notions of the change of water into air, and air into water, had no reference to that particular gas, but first to *nitrogen*, and afterwards to a mixture of gases, the chief of which was *carbonic oxide*\*—M. Arago will keep you “*waiting*” long before he rejoins you in the advocacy of any part of the sup-

\* Report of the British Association for 1839, p. 27.

posed claims of your client, or thanks you for classing him with yourself as still cherishing the conviction that "Mr Watt had, unknown to Cavendish, anticipated his great discovery."

That which renders the self-devotion of this knight-errantry complete, is the singular fact that you are fighting for Watt *against himself*. I had formerly come to the conclusion that he never thought of claiming the discovery in the sense which you suppose, nor in any other respect than as regards the theory of the extrication of heat and light from the combining gases; and a circumstance has lately been pointed out to me by a friend, which establishes this conclusion.

The edition of Robison's Mechanical Philosophy, published by Sir D. Brewster, was revised by Watt himself. In that revision we find him by no means indifferent to his own just fame. Writing to the Editor he says, "I have carefully perused my late excellent friend Dr. Robison's articles, 'Steam and Steam-Engines,' in the *Encyclopaedia Britannica*, and have made remarks upon them in such places, where either from the want of proper information, or from too great a reliance on the powers of his extraordinary memory at a period when it probably had been weakened by a long state of acute pain, and by the remedies to which he was obliged to have recourse, he had been led into mistakes in regard to facts, and also in some places where his deductions have appeared to me to be erroneous. Dr. R. qualifies me as 'the pupil and intimate friend of Dr. Black:' he afterwards, in his dedication to me of Dr. Black's Lectures upon Chemistry, goes the length of supposing me to have professed to owe my improvements upon the steam-engine to the instructions and information I had received from that gentleman, which was certainly a misapprehension; as though I always felt and acknowledged my obligations to him for the information I had received from his conversation, and particularly for the knowledge of the doctrine of latent heat, I never did nor could consider my improvements as originating in those communications. He is also mistaken in his assertion, p. 8 of the preface to the above work, that I had attended two courses of the Doctor's lectures; for unfortunately for me, the necessary avocations of my business pre-

vented me from attending his or any other lectures at College." Mr. Watt then quotes from these lectures a passage in which Black is made to say, " My own fortunate observation of what happens in the formation and condensation of steam, had suggested to my friend Mr. Watt his improvements in the steam-engine," and remarks, " it is very painful to me to controvert any assertion or opinion of my revered friend ; yet in the present case I find it necessary to say that he appears to have fallen into an error\*."

But in revising the article on Steam, and making remarks on those places in which Dr. Robison had been led into mistakes, Watt makes no remark on the following very decisive passage :—" We know that in vital or atmospheric air there is not only a prodigious quantity of fire which is not in the vapour of water, but that it also contains light, or the cause of light, in a combined state. This is fully evinced by the great discovery of Mr. Cavendish of the composition of water: there we are taught that water, and consequently its vapour, consists of air from which the light and greatest part of the fire have been separated ; and the subsequent discoveries of the celebrated Lavoisier show that almost all the condensable gases with which we are acquainted, consist either of airs which have lost much of their fire, and perhaps light too, or of matters in which we have no evidence of light and fire being combined in this manner."

Thus you see, that jealous as Watt appears of any undue share in his own discoveries being attributed even to his "revered friend" Dr. Black, he allows "the great discovery of the composition of water" to be assigned to Cavendish without reclaiming the least participation in it for himself.

These extracts entirely relieve his memory from any suspicion of his having been a party to the erroneous statements contained either in the article 'Water' in the first edition of the

\* I conceive Watt to mean that the *facts* known to him respecting the condensation of steam, independent of Black's *theoretical explanation* of them, were the foundation of his improvements ; and I am bound therefore, on his own showing, to allow that M. Arago has done right in not placing the merit of Watt in the study and application of *abstract philosophical principles*, so much as in ingenuity of mechanical contrivance and the happy use of well-observed facts.

*Encyclopædia Britannica*, to which you have referred, or in the posthumous lectures of Black. Nor do I hold Black responsible for the fabulous history of this discovery given in the latter work. It is well known that that unambitious man left behind him no MSS. of any account, and that the Lectures published under his name were chiefly composed out of the reminiscences of the able but incorrect Editor. Robison, on historical points, was a very inaccurate writer; and to his inaccuracy I attribute the extraordinary string of errors on this subject which I have formerly pointed out.

It is from the latter work that you seem to have taken your supposed facts; and you have in consequence entirely misstated the nature of Cavendish's experiments. Where, allow me to ask, do you find in his paper, or his notes, any such matter as this? "He then weighed accurately the air of both kinds, which he exposed to the stream of electricity\*; and he afterwards weighed the liquid formed by the combustion: he found that the two weights corresponded with great accuracy" (*Life of Cavendish*, p. 433): and again, "Water equal to the weight of the two gases taken together remained as the produce of the combustion." *Cavendish made no such experiments*; as you will find whenever you take the trouble to read either the documents themselves†, or my account of them‡. I have already stated that *this* method of determining the composition of water, which is attended with great practical difficulties, was tried indeed at a later time by the French philosophers with such accuracy as it admits of, but that Cavendish, with his usual sagacity, had taken an easier and more certain road: having mastered beyond any of his contemporaries the analysis of gases, and possessed himself of their specific properties, he was enabled to substitute the method of volume for that of weight; he

\* Here is a double confusion. By exposing common air to "the stream of electricity," Cavendish composed *nitric acid*; but he composed *water* by simply firing the component gases: and in both cases it was not the *weight* of the product, but the *nature* of it, which from the form of the experiments he had to determine, and did determine.

† *Phil. Trans.*, vol. lxxiv. *Experiments on Air*, by H. Cavendish, Esq. Report of the British Association for 1839, autograph notes of experiments.

‡ Report of the British Association for 1839, pp. 35, 36.

ascertained that when a certain measure of hydrogen was burnt with a certain measure of common air containing a known proportion of oxygen, the whole of the oxygen and hydrogen disappeared, no loss of weight ensued, and *pure water* was the result. To draw from these premises the obvious conclusion, there was no need to weigh the airs, or to compare their weight with that of the water that lined the glass after combustion; and he did *not* compare it. Lavoisier followed in his steps: and should you ever read his papers, you will find that *he* too in the first instance contented himself with *deducing* the equality of the weights as a *corollary* from experiments of the same kind as those of Cavendish.

Had you happened to consult the *second* edition of the *Encyclopædia Britannica* as well as the *first*, you would have found it purged both of these, and some other of Robison's historical mistakes. You would have found all that you have referred to *omitted*; and in the article 'Chemistry,' compiled under the revision of friends and connections of Watt, the following account substituted in its place. "In the year 1781, Mr. Cavendish proved that water is not a simple element, but that it is composed of pure or vital air, and inflammable air." "In the mean time the French chemists were not idle; the celebrated Lavoisier, in conjunction with some of his philosophical friends, confirmed by the most decisive experiments the truth of *Mr. Cavendish's discovery of the composition of water*, which was now received and adopted by almost every chemist." A detailed account is then given of Cavendish's experiments; and it is added, "*these experiments were made in 1781, and they are undoubtedly conclusive of the composition of water.* It would appear that Mr. Watt entertained the same ideas on this subject. When he was informed by Dr. Priestley of the result of *these experiments*, he observed, Let us consider what obviously happens in the deflagration of oxygen and hydrogen gases," &c. "Thus it appears that Mr. Watt had *a just view of the composition of water, and of the nature of the process by which its component parts pass to a liquid state from that of an elastic fluid.*"

In this account the ideas entertained by Mr. Watt obtain more notice perhaps than would have been accorded to them by an in-

different historian; but the statement of the discovery is correct, as is also that of the view which Watt took of the subject, if we confine the assertion of the justness of his ideas to his apprehension of the relation of Cavendish's discovery to certain *theories of light and heat*; for of the *material base* of water he had certainly no just conception when he wrote the letter which is quoted above. I have shown that his views in 1783 and 1784 were founded on several suppositions:—1st, that Priestley had converted water into *atmospheric air*; 2nd, that he had obtained a weight of water equal to the weight of a mixture of oxygen with *the gases extricated by heat from moist charcoal*; 3rd, that he had shown good reason to believe that *carbon*, combined in a certain proportion with oxygen, constitutes water. All these suppositions agreed perfectly with the opinions which Watt really expressed, that water was formed of *dephlogisticated air* and *phlogiston*; but no one of them is consistent with the opinions attributed to him by an erroneous translation of his words, that water is formed by the combination of oxygen with *hydrogen gas*\*.

From your mention of Sir H. Davy's sentiments without a quotation, I suppose that he, like Dr. Henry, has been among the number of those on whose attention this untenable claim has been privately pressed; all I know of Davy's opinion on the subject is from his *published works*, in which he has spoken, like other chemists, of the composition of water and of nitric acid as “*the two grand discoveries of Mr. Cavendish*.” But in referring to the name of this much-honoured and regretted friend, I must take the opportunity of noticing what I think a serious error in your impressions respecting one point in his personal character. You begin your sketch of his life with these words: “Sir H. Davy being now removed beyond the reach of such feelings, as he ought always to have been above their influence, that may be said without offence of which he so disliked the mention; he had the honour of raising himself to the highest place among the chemical philosophers of the age, emerging by his merit alone from an obscure condition.” A simple anecdote may suffice to set his feelings on this subject in a more favourable light. When Davy was exhibiting to myself and three

\* See Report of the British Association for 1839, pp. 24, 25.

others the discoveries which he had then recently made relative to his safety-lamp, and when those present, among whom were the Hanoverian minister and the late Lord Lonsdale, were highly admiring the beauty of his experiments, with still higher admiration I heard him reply, “Yes, I have some reason to be proud of them, for my experiments on flame were first made *with a tallow candle in an apothecary's shop.*”

In these slight sketches which you have given us of the history of men eminent in science, there is one other scientific subject besides the discovery of the composition of water, on which you appear to have bestowed some consideration, namely,—the first discoveries of the gases. Here Cavendish is still out of favour with you. You pluck another feather from his wing; and having made a present of the discovery of water to Mr. Watt, dispense that of hydrogen gas to Dr. Black.

“The nature of hydrogen,” you say, “was perfectly known to him, and both its qualities of being inflammable, and of being so much lighter than atmospheric air; for as early as 1766 he invented the air-balloon, showing a party of his friends the ascent of a bladder filled with inflammable air: Mr. Cavendish only more precisely ascertained its specific gravity, and showed, what Black could not have been ignorant of, that it is the same from whatever substance it is obtained\*.”

You ought to have recollected, when again contravening the received opinion of chemists†, your own remarks on the supposed omission of Cavendish to state exactly the time when he had communicated to Priestley his experiments on the composition of water. “*Dans une addition de Blagden faite avec le consentement de Cavendish, on donne aux expériences de ce dernier le date de l'été de 1781.* On cite une communication de [à] Priestley, *sans en préciser l'époque*, sans parler de conclusions, sans même dire quand ces conclusions se pré-

\* Life of Black, p. 383.

† The received account of the discovery of hydrogen is this:—“Its combustible quality is described in the works of Boyle and Hales, of Boerhaave and Stahl; but it was not till the year 1766 that its properties were particularly ascertained, and the difference between it and atmospheric air pointed out by Mr. Cavendish.”—Encycl. Brit., Art. Chemistry, 1810.

sentaient à l'esprit de Cavendish. Ceci doit être regardé comme une très grosse omission (*a most material omission* \*).” Nothing indeed can be more unfounded than this animadversion. In the passage to which you refer, the words of Cavendish are these:—“ All the foregoing experiments on the explosion of inflammable air with common and dephlogisticated airs, except those which relate to the cause of the acid found in the water, were made by me in the summer of 1781, and mentioned by me to Dr. Priestley, *who in consequence of it made some experiments of the same kind, as he relates in a paper printed in a preceding volume of the Transactions.*” Now what language could claim and prove priority more precisely than this? Priestley addressed this paper to the Royal Society on the 21st of April 1783; the communication of Cavendish's experiments, acknowledged in it as having suggested his own, must have been prior to the speculations founded thereon, which Watt addressed to Priestley on the 26th of the same month, as well as to Lavoisier's experiments which followed in June. Moreover, in this paper, Priestley describes the experiments communicated to him, as “ Mr. Cavendish's experiments on *the reconversion of air into water*,” a description which involves in its very expression the whole *conclusion* in question.

But though this “*most material*,” or in M. Arago's translation, this “*grosse*” omission turns out to be *none*, you ought, I repeat it, to have remembered your own demand for precision of dates, when you ascribed to Black a prior knowledge of the distinguishing properties of hydrogen gas. In proof that Black knew before Cavendish that this gas is “*so much lighter than atmospheric air*,” you allege, that “as early as 1766 he invented the air-balloon, showing a party of his friends the ascent of a bladder filled with inflammable air: Mr. Cavendish only more precisely ascertained its specific gravity.”

As early as 1766?—Are you not aware that Cavendish's paper on factitious airs was published in this year? Is it not a “*most material omission*” that you have forgotten “*préciser l'époque*” of Black's experiment with the balloon, so as to

\* Historical note, Life of Watt, p. 383.

show whether it was before or after the publication of Cavendish's paper? Leslie tells the story of the balloon somewhat differently from you.—“The late most ingenious and accurate Mr. Cavendish, in 1766, found, by a most nice observation, this fluid to be at least seven times lighter than atmospheric air. It *therefore* occurred to Dr. Black of Edinbro', that a very thin bag filled with hydrogen gas would rise to the ceiling of a room. He provided accordingly the allantois of a calf, with a view of showing at a public lecture such a curious experiment before his numerous auditors; but owing to some unforeseen accident or imperfection it chanced to fail, and that celebrated Professor, whose infirm state of health and indolent temper more than once allowed the finest discoveries when almost within his reach to escape his penetration, did not attempt to repeat the exhibition, or seek to pursue the project any further\*.”

If you are dissatisfied with Leslie's version of your anecdote, let me refer you to other authorities. In one of those articles of the *Encyclopædia Britannica* which are stated to have been composed or revised by Professor Miller, Dr. Muirhead, and Sir David Brewster, the circumstance is thus narrated:—“In the year 1766 Mr. Henry Cavendish ascertained the weight and other properties of this gas, determining it to be at least seven times lighter than atmospheric air. *Soon after which* it occurred to Dr. Black that perhaps a thin bag filled with hydrogen gas might be buoyed up by the common atmosphere.”

I hope I have now illustrated sufficiently the value of the canon of criticism which you have laid down. These delicate inquiries,—that nothing is so necessary as “*préciser l'époque*.”

“Cavendish,” you say, “only more precisely ascertained the specific gravity of inflammable air; and showed, *what Black could not be ignorant of, that it is the same from whatever substance it is obtained*.” Now, in the first place, *inflammable air* is not the same from whatever substance it is obtained. This was the error into which Priestley fell when he attempted to repeat Cavendish's experiments on “the reconversion of air into water,” and prepared his *dry inflammable air* from

\* Supp. to Encycl. Brit., Art. Aéronautics.

*moist charcoal*; this was the error under which Watt laboured when he concluded that water consists of "*this kind of air*" combined with air deprived of phlogiston; this was the error which nullified the researches of the one and the speculations of the other: here lay a distinction, the knowledge of which qualified Cavendish to make this discovery, the ignorance of which disabled Priestley and Watt from confirming it, and the inattention to which has misled M. Arago and yourself into giving weight to their fruitless attempts. But supposing you to mean "that inflammable air is the same, *whether obtained from zinc or iron*," why do you say that Black could not be ignorant of *that*? How do you think he was to know it? How did Cavendish know it? He tells you that he learnt it by having ascertained by experiment that the specific gravity of the gas from either material was the same. Had Black ascertained this? Had he any test whatever by which he could know that these gases were the same?

But Cavendish "*only more precisely ascertained the specific gravity of inflammable air!*" If any person conversant in the history of pneumatic discoveries were to be asked to enumerate the most important of the early advances in that branch of science, he would certainly name—1st, the discovery of the weight of the air by Galileo; 2nd, the discoveries of its law of compression\*, and of the factitious gases, by Boyle; 3rd, the theory of the fixation of gases by chemical attraction, propounded by Newton; 4th, the discovery of specific and elective affinities in one of those gases, by Black; 5th, the discovery of the difference of specific gravity in several gases, by Cavendish.

You do not distribute their honours to any of these great discoverers with a severe attention to matter of fact; but I must do you the justice to own that you preserve a principle of equity in your adjudications. You omit, it is true, to dwell upon, or even to mention, the main point of novelty in the researches of Black; but then you give to Black the discoveries

\* The first statement of this law, commonly called the law of Mariotte, who only helped to confirm it, was given by Boyle with a series of experiments exhibiting a close correspondence with it in his *Defensio Doctrinæ de Aëris Elatéro contra Linum*, published in 1662.

of Boyle and Cavendish, and make it up to Cavendish by allowing him the merit which belongs to Galileo.

For Cavendish you say, “He carried his mathematical habits into the laboratory; and not satisfied with showing the other qualities which make it clear that these two aëriform substances are each *sui generis*, and the same from whatever substances, by whatever processes they are obtained,—not satisfied with the mere fact that one of them is heavier, and the other much lighter than atmospheric air,” (a previous acquaintance with all which facts you have taken care to ascribe to Dr. Black) “he inquired into the precise numerical relation of their specific gravities with one another and with common air, and *first showed an example of weighing permanently elastic fluids*: unless indeed Torricelli may be said before him to have shown the relative weight of a column of air and a column of mercury, or the common pump to have long ago compared in this respect air with water. It is however sufficiently clear that neither of these experiments gave the relative measure of one air with another; nor indeed could they be said to compare common air with either mercury or water, although they certainly showed the relative specific gravity of the two bodies, taking air for the middle term or common measure of their weights.”

What a strange *qualification* of a still stranger *assertion*! If instead of this confusion between specific gravities and equivalent columns, ending with the grave suggestion, that “the relative specific gravities of water and mercury” might have been taken by the intermedia<sup>tion</sup> of “air,” you had said that philosophers have attempted, from the relative heights of the barometer at different elevations, to calculate the mean specific gravity of the atmosphere\*, there would have been mean-

\* The following quotation will show the nature of these calculations (Dan. Bernoulli Joh. Fil. *Hydrodynamica*, *Argentorati*, 1738. Sect. 10. 16. p. 209):—“Patet exinde quid censendum sit de illa methodo qua in Anglia aliquando usos esse recenset D. Du Hamel, in *Hist. Acad. Sc. Paris*. ad indagandam rationem inter gravitates specificas aëris et mercurii: observata nimis altitudine mercurii in loco humiliori, tum etiam in altiori, gravitates specificas in aëre et mercurio statuerunt, ut erat differentia altitudinum mercurii in barometro ad altitudinem inter locos observa-

ing at least in the *qualification*. But then what an *assertion* to hazard ! considering the great number of experiments extant for the *direct* determination of the weight of air compared with that of water, first instituted by Galileo, and then repeated successively by Descartes, Mersenne, Boyle, Hook, Newton, Cotes, and lastly by Hawksbee, whose determina-

tionum interceptam. Etiam si aëris ejusdem densitatis ponatur ab imo observationis loco ad alterum usque, non licet tamen inde judicare de ejus gravitate specificâ, ratione mercurii. Quicquid ab experimento colligere licet hoc solum est :—

“ Consideremus scilicet integrâ crustam aëream terram ambientem atque inter ambo observationis loca interceptam, et erit pondus istius crustæ ad superficiem terræ ut pondus columnæ mercurialis qualis in barometro descendit ad basin ejus; manifesta hæc sunt ex eo quod summa basium A et B sustinet quidem summam ponderum quæ habent columnæ aëreæ A C et B D, neque tamen quævis basis premitur suæ columnæ pondere seorsim, et quod idem, resectis columnis A g et B h, intelligi debet de columnis g C et h D diaphragmatis in g et h positis incumbentibus. Igitur experimentum non tam gravitatem specificam aëris in quo factum est indicat, quam omnis aëris terræ proximi gravitatem specificam medium determinat; prior admodum variabilis est; altera procul dubio constanter eadem fere permanet.

“ Faciamus computum gravitatis specificæ istius mediae aëris omnis qui terram ambit. Multis vero experimentis, quæ in diversis locis parum supra mare elevatis sumpta fuerunt, id constat, elevationi 66 pedum proxime descensum respondere unius lineæ in barometro. Sequitur inde quod aëris gravitas specifica media, ratione mercurii, sit ut altitudo unius lineæ ad altitudinem 66 pedum, *i. e.* ut ut 1 ad 9504, ergo, posita gravitate specifica mercurii = 1, erit gravitas specifica media aëris = 0.00105. Notabile est profecto tantam esse hanc gravitatem medium aëris: certus enim sum, vel maxime sœviente hic locorum frigore, aëris gravitatem specificam vixduin tantam esse quantam nunc exhibuius pro statu medio omnis aëris terram ambientis: at sub æquatore multo erit minor, et omnibus recte perpensis non crediderim gravitatem medium aëris qui inter utramque latitudinem 60 gr. continetur, ultra 0.000090 excurrere; quo posito erit gravitas media aëris ab utroque polo ad 30 gradus terram cingentis, quod spatium paullo plus quam octavam totius terræ superficie efficit partem, = 0.000210, quæ dupla est aëris hic locorum densissimi: sub ipso autem polo, præsertim antarctico, admodum gravior erit aëris, et fortasse aquâ vix decies levior, cum est frigidissimus atque densissimus.

“ 32. Et quia aëris mediocriter densi gravitas specifica est ad gravitatem specificam merc. ut 1 ad 11000, ipsaque altitudo media merc. in barometro, pro locis parum a superficie maris elevatis, sit  $2\frac{1}{2}$  ped. Paris., erit altitudo aëris homogenii mediocriter densi 25066 pedum.”

tion was assumed and quoted by Cavendish himself for the purpose of comparing the specific gravity of common air with those of the factitious gases,—it is a strong instance of the kind of equity for which I have given you credit, that you should have allotted to Cavendish the merit of having “*first showed an example of weighing permanently elastic fluids.*” Even Descartes allowed that Galileo’s “method of weighing the air was not amiss\*;” and the experiments of the great Italian philosopher, which laid the original foundation of all our knowledge of elastic fluids, ought not to have been entirely forgotten by any one who appreciates duly those capital discoveries by which the ideas of men are fixed and a new order of facts is ascertained.

\* “*Si la façon de peser l’air n’est pas mauvaise, si tant est que la pesanteur en soit si notable qu’on la puisse apercevoir par ce moyen; mais j’en doute.*” (*Oeuvres de Descartes*, tom. vii. p. 440.) Thus Descartes wrote to Mersenne in 1638. In 1642 he repeated the experiment himself by a method far less susceptible of accuracy, and obtained a result much further from the truth, which satisfied him however, “*que la poids de l’air est sensible en cette façon.*” (*Oeuvres*, tom. viii. p. 567.) Dr. Whewell has taken notice (*History of Mechanics*, p. 66) that “in a letter of the date of 1631 he (Descartes) explains the suspension of mercury in a tube closed at the top by the pressure of the column of air reaching to the clouds.” In this letter the atmosphere is compared to a pack of wool, the filaments of which are all heavy, and press on each other from the clouds to the earth, being only kept apart by the æther which plays between them, “*ce qui fait un grand pesanteur*”—expressions which at first sight might lead to the idea that he had anticipated the theory of the elevation of the barometric column; but it is evident from many subsequent letters of Descartes, that he had no correct conception of the statical pressure of fluids, and was therefore incapable of reasoning justly on this subject. The tube in which the mercury was suspended in the case in question, was *a straight tube without a basin*: he tried to account for the phænomenon of its suspension on his principle of *circular movement in a plenum*, by supposing that the mercury, before it could quit the tube, must commence the circle of motions required to bring down from the sky a current of æther to supply the vacuum left at the top of the tube by the descent of the quicksilver; and presuming the column of air which it had to lift to be as heavy as itself, he concluded that no such circular motion in the chain of matter could take place. It is possible however that this representation of the atmosphere as a heavy equiponderant column, though wanting the conception of equal pressure, may have conducted to suggest the more correct views of the subject afterwards adopted.

To Black, on the other hand, with like even-handed justice, you ascribe a knowledge of the lightness of hydrogen and the heaviness of carbonic gas, which you have no ground for suspecting him to have possessed. Experiments, indeed, had been made with a view of ascertaining such points; and your assertion, that "Cavendish first set the example of weighing permanently elastic gases," is so far from the truth, that the factitious gases themselves had been weighed both by Hawksbee and Hales. Hales weighed the "*air of tartar*," which consists of a mixture of carburetted hydrogen and carbonic gases, in a bladder, and then filling it with common air compared the weights\*; Hawksbee ascertained accurately the specific gravity of air that had passed through tubes filled with iron wires, and heated red in the fire, which consisted partly of carbonic acid and partly of nitrogen†. But these mixed gases approached too nearly to common air in weight to enable the experimenters to establish a distinction. An attempt too had been made by Greenwood, a Professor of Mathematics at Cambridge in New England, to ascertain the specific gravity of the deleterious air in a well, which was doubtless chiefly carbonic gas; but the method employed by him was not sufficiently delicate to show a difference of density. Such was the state of knowledge, or rather ignorance, on this subject previous to the experiments of Cavendish. We have not the least reason to believe that any one had observed the different weights of the different kinds of air. Dr. Mayow‡ indeed about a century before had supposed his "*nitro-igneous aura*," to the combinations of which he ascribed the phænomena of acidification, combustion and vitality, to be *heavier* than the residual air from which it is separated in those processes; and this opinion, which proved to be correct, he entertained so distinctly, as to represent the specific lightness of the vitiated air, after it had served its purpose of sustaining life, as a provision of nature for freeing us from a noxious atmosphere. But he had no better ground for entertaining such an opinion than his observation of the movements of animals which he had confined in a close vessel, and which appeared in his experiments to seek for a less

\* Analysis of the Air, p. 185. † Phil. Trans., No. 328, p. 199.

‡ *De parte aëria ignaque Spir. Nitri.*

suffocating air in the lower part of the receiver, whilst they avoided the upper.

Such loose surmises as these detract nothing from the great experimental discovery of Cavendish, the importance of which cannot be better expressed than in the words of an eminent chemist and chemical historian\*—"It can scarcely be said that pneumatic chemistry was properly begun till Mr. Cavendish published his valuable paper on Carbonic Acid and Hydrogen Gas, in the year 1766." On the fruits of this discovery, in the hands of its author and of all succeeding chemists, and its consequences to the study of gaseous substances and their combinations, I need not dwell. It is enough to remark, that the ascertainment of *this physical difference* in the gases was the first *conclusive proof of a plurality of elastic fluids*.

Another point of no small consequence to pneumatic chemistry was first made out in this paper. From the earliest discovery of factitious airs, it had been observed that a considerable portion of several of these disappeared after they had been generated, though there had been no change of temperature or pressure. The usual statement of this phænomenon was, that the elasticity of the air had been *destroyed*. Dr. Hales, dissatisfied with so loose an explanation, accounted for the loss, which in the case of nitrous acid *he* first observed, after the following manner:—"When fresh air is let into the receiver, whose included air is impregnated with the fumes arising from the mixture of compound *aqua-fortis*, or spirit of nitre, and Whitstable pyrites, mentioned in the following experiment, then the air in the receiver turns very red and turbid, and much air is absorbed after several repeated admissions. When fresh air is thus admitted into the glasses full of sulphureous, though clear, air, a good many particles of the fresh air must needs be reduced by the sulphureous ones from an elastic to a fixed state, as in the effervescences of other liquors. Therefore the rising of the water in the glass vessel does not seem to be wholly owing to the rebating of the air's elasticity in some degree, but rather to the reduction of it from an elastic to a fixed state, which is further probable from hence, viz. that the whole quantity of air admitted at several

\* Dr. T. Thomson's Biographical Account of Priestley, Ann. Phil., vol. i. p. 91.

times is equal, or nearly equal, to the quantity of sulphureous\* air A. Z., so that *both airs are at the same time contained within the space A. Z.*"

In this important observation, subsequently turned to such good account by Priestley and Cavendish, Hales gave the true theory of the loss of volume which occurs by the admission of common air to nitrous gas; but the variable, and apparently capricious, loss of elasticity which he, with others, had remarked in other gases, he could not explain. "Though a good part of the air," he says, "which rises from *fluids* seems to have existed in an elastic state in those fluids, yet the air which arises from *solid* bodies, either by the force of fire or effervescence, does not seem to arise only from the interstices of those bodies, but principally from *the most fixed parts* of them. For since the airs which are raised by the same acid spirit from a vast variety of substances have very different degrees of permanency, as was shown in Exp. 10, No. 3, 4, 5, 6, and in Exp. 11, No. 6, 7, 8, 9, 10 of experiments on stones, hence it is probable that these airs do not arise from latent interstices of the dissolved stones, &c., but from the solid fixed particles of them; and since the whole of some of these newly-generated airs does in a few days lose its elasticity, it should seem hence probable, that whatever air arises from the spirit in the effervescence is not permanently elastic, or else that in the rotation of some stones it is thrown off into a more permanently elastic state than from others."

The cause of this loss of volume was first explained in Cavendish's paper: he proved by experiment, that carbonic acid is condensed over water, but not over mercury. You indeed tell us that Black "*found this gas incondensable*;" but he has nowhere told us as much himself; and you might with more safety have presumed the contrary; the true statement being, that he and his predecessors had found it *condensable*, and that Cavendish found the conditions under which *it is*, and under which (at a given temperature and pressure) *it is not condensed*.

In the same spirit of liberality you take "*the capital discovery*," that the air of the atmosphere is not the only air per-

\* The appellation *sulphureous*, as used by the earlier chemists, is a general and theoretical term, analogous to *phlogistic* in the succeeding theory.

manently elastic, from its ancient owners, to appropriate it to Black, and expend much learned pains in setting forth the originality and importance of the “*doctrine*” which you ascribe to him. “The great step,” you say, “was now made, that the air of the atmosphere is not the only permanently elastic body, but that others exist, having perfectly different qualities from atmospheric air, and capable of losing their elasticity by entering into chemical union with solid and with liquid substances, from which, being afterwards separated, they regain the elastic or aërisiform state.”.....“In order to estimate the importance of this discovery, and at the same time to show how entirely it altered the whole face of chemical science, and how completely the doctrine was original, we must now examine the state of science which philosophers had previously attained to. It has often been remarked, that no great discovery was ever made at once, except perhaps that of logarithms: all have been preceded by steps which conducted the discoverer’s predecessors nearly, though not quite, to the same point. Some may perhaps think that Black’s discovery of fixed air affords no second exception to this rule; for it is said that Van Helmont, who flourished at the end of the sixteenth and beginning of the seventeenth century, had observed its evolution during fermentation, and gave it the name of *gas sylvestre*, spirit from wood, remarking that it caused the phænomena of the Grotto del Cane near Naples; but though he, as well as others, had observed an aërisiform substance to be evolved in fermentation and in effervescence, there is no reason for affirming that they considered it as differing from atmospheric air, except by having absorbed or become mixed with various exhalations or impurities. Accordingly a century later than Van Helmont, Hales, who made more experiments upon air than any of the old chemists, adopts the commonly received opinion, that all elastic fluids were only different combinations of the atmospheric air with various exhalations or impurities: and this was the universal opinion upon the subject, both of philosophers and the vulgar.”.....“It is now fit that we see in what manner the subject was treated by scientific men at the period immediately preceding Black’s discoveries. The article ‘Air,’ in the French *Encyclopédie*, was published in 1751, and written by D’Alembert himself. It is,

as might be expected, able, clear and elaborate. He assumes the substance of the atmosphere to be alone entitled to the name of air, and to be the foundation of all other permanently elastic *bodies*. When D'Alembert wrote this article, he gave the doctrine then universally received, that all the other kinds of air were only impure, and that this fluid alone was permanently elastic, all other vapours being only like steam, temporarily aërisform. Once the truth was made known, that there are other gases in nature, only careful observation was required to find them out\*."

After all this, should I venture to affirm that you have postdated our knowledge of permanently elastic gases, other than the atmosphere, by about a hundred years,—were I to suggest that in this case also the old story is the true one, and that Priestley has correctly recorded the real historical fact when he said, "Mr. Boyle, I believe, was the first who discovered that what we call fixed air, and also inflammable air, are really elastic fluids capable of being exhibited in a state unmixed with common air,"—were I to add that the existence of various elastic fluids was generally recognised by the philosophers of Europe, and particularly by those whom you have quoted as instances to the contrary, during the century which preceded Black's essay, as distinctly, and more distinctly, than by Black himself,—I know not what you would think of me. Nevertheless, since this is a passage in the history of science which deserves to be told with a strict regard to dry matter of fact, I must beg you to listen with patience to an account of it certainly very different from your own.

It was in December 1659 that Boyle published his "New Physico-mechanical Experiments," among which is to be found a description of two of those gases separable from fixed bodies, which he subsequently denominated *factitious airs*. The high interest which may be justly attached to all the circumstances of discoveries so important as this, induces me to give the details of it in the words of the author.

"Contenting myself," he says, "to have mentioned our author's (Kircher's) experiment as a plausible, though not demonstrative, proof that water may be transmuted into air, we

\* Life of Black, pp. 331-36.

will pass on to mention, in the third place, another experiment which we tried in order to the same inquiry. We took a clear glass bubble, capable of containing by guess about three ounces of water, with a neck somewhat long and wide of a cylindrical form: this we filled with oil of vitriol and fair water, of each almost a like quantity, and casting in half a dozen small nails we stopped the mouth of the glass, which was top-full of liquor, with a flat piece of *dia palma* provided for the purpose, that, accommodating itself to the surface of the water, the air might be exquisitely excluded; and speedily inverting the phial, we put the neck of it into a small wide-mouthed glass that stood ready with more of the same liquor to receive it. As soon as the neck had reached the bottom of the liquor it was dipped into, there appeared at the upper part, which was before the bottom of the phial, a bubble of about the bigness of a pea, which seemed rather to consist of small and recent bubbles produced by the action of the dissolving liquor upon the iron, than any parcel of the external air that might be suspected to have got in upon the inversion of the glass, especially since we gave time to those little particles of air which were carried down with the nails into the liquor to fly up again. But whence the first bubble was produced is not so material to our experiment, in regard it was so small; for soon after we perceived the bubbles produced by the action of the menstruum upon the metal, ascending copiously to the bubble named, and breaking did soon exceedingly increase it, and by degrees depress the water lower and lower, till at length the substance contained in these bubbles possessed the whole cavity of the glass phial, and almost of its neck too, reaching much lower in the neck than the surface of the ambient liquor wherewith the open-mouthed glass was by this means almost replenished. And because it might be suspected that the depression of the liquor might proceed from the agitation whereinto the exhaling and imprisoned steams were put by that heat which is wont to result from the action of corrosive salt upon metals, we suffered both the phial and the open-mouthed glass to remain as they were in a window for three or four days and nights together; but looking upon them several times during that while, as well as at

the expiration of it, the whole cavity of the glass bubble and most of its neck seemed to be possessed by air, since by its spring it was able for so long to hinder the expelled and ambient liquor from regaining its former place. And it was remarkable that just before we took the glass bubble out of the other glass, upon the application of a warm hand to the convex part of the bubble, the imprisoned substance readily dilated itself like air, and broke through the liquor in divers bubbles succeeding one another.

“ Having also another time tried the like experiment with a small phial and with nails dissolved in *aquafortis*, we found nothing incongruous to what we have now delivered. And this circumstance was observed, that the newly-generated steams did not only possess almost all the whole cavity of the glass, but divers times without the assistance of heat of my hand did break away in large bubbles through the ambient liquor into the open air: so that the experiments with corrosive liquors seemed manifestly to prove, though not that air may be generated out of water, yet that in general air may be generated anew.

“ Lastly, to the foregoing arguments from experience, we might easily subjoin the authority of Aristotle and of his followers the schools, who are known to have taught that *air and water*, being symbolizing elements in the quality of moisture, are *easily transmutable into each other*\*; but we shall rather to the foregoing argument add *this*, drawn from reason—that if, as Leucippus, Democritus, Epicurus and others, followed by divers modern naturalists, have taught, the difference of bodies proceeds but from the various magnitudes, figures, motions, and textures of the small parts they consist of (all the qualities that make them differ being deducible from thence), there appears no reason why *the minute parts of water, and other bodies, may not be so agitated and connected as to deserve the name of air*; for if we allow the Cartesian hypothesis, according to which the air may consist of any terrene or aqueous corpuscles, provided they be kept swimming in the interfluent celestial matter, it is obvious that air

\* This I presume is the *hypothesis, doctrine, or theory*, which Cavendish has been so shrewdly suspected of deriving from *Watt*.

may be as often generated, as terrestrial particles, minute enough to be carried up and down by the celestial matter, ascend into the atmosphere. And if we will have the air to be a congeries of little slender springs, it seems not impossible, though it be difficult, that the small parts of divers bodies may, by a lucky concourse of causes, be so connected as to constitute such little springs, since water in the plants it nourisheth is usually contrived into springy bodies, and even the bare altered position and connexion of the parts of a body may suffice to give it a spring that it had not before, as may be seen in a thin and flexible plate of silver, into which, by some strokes of a hammer, you may give a spring; and by only heating it red-hot, you may make it again as flexible as before.

“These, my Lord, are some of the considerations at present occurring to my thoughts, by which it may be made probable that air may be generated anew.”

In a subsequent part of the same treatise, Boyle adds an account of another discovery of a similar kind. “I took,” he says (exp. 42), “whole pieces of red coral, and cast them into as much spirit of vinegar as sufficed to swim about an inch over them: these substances I made use of that the ebullition upon the solution might not be too great, and that the operation might last the longer.” It gave but few bubbles, till the receiver under which it was placed was exhausted; “then the menstruum appeared to boil in the glass like a seething-pot. To avoid suspicion, that these proceeded not from the action of the *menstruum* upon the coral, but from the sudden emersion of those many little parcels of air that are wont to be dispersed in liquors, we conveyed over distilled vinegar alone into the *receiver*, and kept it awhile there to free it from the bubbles, which were but very small, before ever we put the coral into it. The former experiment was another time tried in another small receiver with coral grossly powdered, and the success was much alike.”

Of the two gases thus first obtained and separated, he observed some time afterwards that the one was inflammable\*,

\* “Having provided a saline spirit, which by the uncommon way of preparation was made exceeding sharp and piercing, we put into a phial, ca-

and the other liable, in part, to lose its elasticity\*; he extended his experiments on the generation of "factitious airs" to a variety of materials, multiplying them to such an ample of containing three or four ounces of water, a convenient quantity of filings of steel, which were not such as are commonly sold in shops to chemists and apothecaries, those being usually not free enough from rust, but such as I had awhile before caused to be purposely filed off from a piece of good steel. This metalline powder being moistened in the phial with a little of the menstruum, was afterwards drenched with more; whereupon the mixture grew very hot, and belched up copious and very stinking fumes, which, whether they consisted altogether of the volatile sulphur of the Mars, or of metalline steams participating of a sulphureous nature, and joined with the saline exhalations of the menstruum, is not necessary here to be discussed. But whencesoever this stinking smoke proceeded, so inflammable it was, that upon the approach of a lighted candle to it, it would readily enough take fire, and burn with a bluish and somewhat greenish flame at the mouth of the phial for a good while together; and that, though with little light, yet with more strength than one would easily suspect. This flaming phial therefore was conveyed to a *receiver*, which he who managed the pump affirmed that about six exsuctions would exhaust. And the receiver being well cemented on, upon the first suck the flame suddenly appeared four or five times as great as before, which I ascribed to this, that upon withdrawing of the air, and consequently the weakening of its pressure, great store of bubbles were produced in the menstruum, which breaking could not but supply the neck of the phial with store of inflammable steams, which as we thought took not fire without some noise. Upon the second exsuction of the air, the flame blazed out as before, and so it likewise did upon the third exsuction; but after that it went out, nor could we rekindle any fire by hastily removing the receiver: only we found that there remained such a disposition in the smoke to inflammability, that holding a lighted candle to it a flame was quickly rekindled."—*New Experiments touching the Relation between Flame and Air*, 1671.

\* "Experiment 8.—A mercurial gauge having been put into a conical glass whose bottom was covered with beaten coral, some spirit of vinegar was poured in, and then the glass stopper closing the neck exactly: on the working of the menstruum on the coral, store of bubbles were for a good while produced, which successively broke in the cavity of the vessel; and their accession compressed the confined air in the closed leg of the gauge three divisions, which I guessed to amount to about the third part of the extent it had before; but some hours after the compression made by this newly-generated air grew manifestly fainter, and the imprisoned gauge-air drove down the mercury again, till it was depressed within one division of its first station; so that in this operation there seemed to have been a double compressive power exercised, the one transient by the brisk agitation of vapours, the other durable from the aerial or springy particles either produced or extricated by the action of the spirit of vinegar on the coral."—*Phil. Trans.* 1675, vol. x.

extent that one of Cotes's hydrostatical lectures is filled with the repetition of them; he remarked the condensability of muriatic acid gas\*, and the orange colour of nitrous acid gas †; and extricated from red lead, by the burning-glass, the gas ‡, which Priestley afterwards having obtained by the same method was led by reasoning from the manner in which red lead is manufactured, to identify with the *dephlogisticated air* of the atmosphere §, just as Mayow before him by a similar inference identified the air fixed in saltpetre with the “*nitro-aërial*

\* “ May 26, 1676.—Sal-ammoniac was put into a receiver with a sufficient quantity of oil of vitriol. Then the air being exhausted, the salt was put into the oil, whereupon a great ebullition presently followed, and the mercury in the gauge showed a good quantity of air to be generated; but this by the same gauge soon after appeared to be destroyed again. The experiment was repeated, and both the production and destruction were slower than before. It was confirmed by these trials that some artificial airs may be destroyed; but why this destruction happens sometimes sooner and sometimes slower, may perhaps seem worthy of further inquiry.”—2nd cont. *Phys. Mech. Expts.* 1676.

† “ We put an ounce of such strong spirit of nitre as is above mentioned into a moderately large bolt head, furnished with a proportionable stem, over the orifice of which we strongly tied the neck of a thin bladder, out of which most part of the air had been expressed, and into which we had conveyed a small phial with a little highly rectified spirit of wine. Then this phial, that was before closed with a cork, being unstopped without taking off the bladder, a small quantity, by guess not a spoonful, of the alcohol of wine was made to run down into the spirit of nitre, where it presently produced a great commotion, and blew up the bladder as far as it would well stretch, filling also the stem and cavity of the glass with very red fumes, which presently after forced their way into the open air, in which they continued for a good while to ascend in the form of an orange-coloured smoke.”—*New Experiments about Explosions*, 1672.

‡ “ September 4, 1678.—I exposed one ounce of minium in an open glass to the sunbeams, concentrated by a burning-glass, and found that it had lost three-fourths of a grain of its weight, though much of the minium had not been touched by the solar rays. May 29.—Repeated the same experiment, in a light glass phial sealed hermetically and exactly weighed, and the loss of weight came to  $\frac{1}{3}$ rd part of a grain. May 30.—I endeavoured to burn the same minium again, but such plenty of air was produced, that the glass broke into a hundred pieces.”—2nd cont. *Phys. Mech. Expts.*

§ “ At the same time that I got the air above-mentioned from mercurius calcinatus and the red precipitate, I had got the same kind from red lead or minium. In this process that part of the minium on which the focus of the lens had fallen turned yellow. The experiment with red lead con-

*aura*" in the atmosphere, which supports combustion and life.

In giving to the gases which he discovered the title of "fictitious airs," Boyle did not confound them with *common air*. The extracts which I have given sufficiently show that he used the word *air* generically, in the sense which he assigns to it in the following passage:—"If I were to allow acids to be one principle, it should be only in some such metaphysical sense as that wherein *air* is said to be one body, though it consist of the associated effluvia of a multitude of corpuscles of very different natures that agree in very little, save in their being minute enough to concur in the composition of a fluid aggregate consisting of flying parts\*."

It would indeed be a great mistake in the history of science, to suppose that the notion of the air being a simple element prevailed among philosophers down to the days of Black. From the time of that remarkable revolution in the scientific mind of Europe which attended the revival of the mechanico-corporeal philosophy, when the phænomena of nature were accounted for no longer by forms and qualities, but by the sizes and motions, the cohesions and disjunctions, of the particles of bodies, the atmosphere came at once to be conceived of as a miscellaneous aggregate of the molecules of a variety of heavy substances thrown into an elastic state, or floating in an active medium of a still finer and more divided consistence.

"Tout corps invisible et impalpable," says Descartes, "se nomme *air*, à savoir en sa plus ample signification†." "By air," says Dr. Wallis, "I find Mr. Hobbs would sometimes have us understand a pure æther, 'aërem ab omni terræ aquæque effluviis purum, qualis putatur esse æther,' to which I suppose answers the *materia subtilis* of Descartes, and M. Hugens's 'more subtile matter' than air: on the other hand, M. Hugens here by air seems to understand that feculent mat-

firmed me more in my suspicion, that the merc. calcinatus must have got the property of yielding this kind of air from the atmosphere, the process by which that preparation and this of red lead is made being similar."—*Priestley's Experiments on Air*, vol. ii. p. 111.

\* *Reflections on the Hypothesis of Alkali and Acidum*, ch. iv. 1676.

† *Œuvres*, tom. vii. p. 237.

ter arising from those the earth's and water's effluvia, which are intermingled with this subtle matter. *We* mean by *air* the aggregate of both these, or whatever else makes up that heterogeneous fluid wherein we breathe, commonly called air, the purer part of which is Mr. Hobbs's air, and the feculent of it is M. Hugens's air\*."

It is curious to trace the fortunes of this *materia subtilis*, from the naked condition in which it was first ushered into notice, to the figure which it now makes in the speculations of science.

Descartes was undoubtedly the first who formed the idea of a liquid medium grosser than heat, but more subtle than air, extending from the heavenly bodies to the earth, filling the aërial interstices with a continuous series of molecular globules, pervading the pores of glass, diamond, and the densest substances, without interruption, and propagating, by communication of impulses from one molecule to another, the movement, or rather the *pressure without locomotion*, simple, and compound, which he considered as constituting *light*†, and *colours*.

\* Extract of Letters from Dr. J. Wallis to the publisher, 1672, Phil. Trans. No. 91.

† Dr. Whewell takes Descartes's "hypothesis concerning light" to have been, "that it consists of small particles *emitted* by the luminous body," and considers this as "the first form of the *emission theory*" (Phys. Optics, ch. x.); and so the theory of the Dioptrics seems to have been understood by some of Descartes's contemporaries; but he explains himself otherwise in his letters. "Je vous prie de considérer que ces petits globes dont j'ai parlé ne sont point des corps qui exhalent et qui s'écoulent des astres jusqués à nous; mais que ce sont des parcelles imperceptibles de cette matière que V. R. appelle elle-même céleste, qui occupent tous les intervalles que les parties des corps transparents laissent entre elles, et qui ne sont autrement appuyées les unes sur les autres que le vin de cette cuve que j'ai pris pour exemple en la page 6 de ma Dioptrique, où l'on peut voir que le vin qui est en C tend vers B, et qu'il n'empêche point pour cela que celui qui est en E ne tend vers A, et que chacune de ces parties tend à descendre vers plusieurs divers endroits, quoiqu'elle ne se puisse mouvoir que vers un seul en même temps. Or, j'ai souvent averti que par la lumière je n'entendois pas tant le mouvement, que cette inclination ou propension que ces petits corps ont à se mouvoir; et que ce que je disois du mouvement, pour être plus aisément entendu, se devoit rapporter à cette propension; d'où il est manifeste que, selon moi, l'on ne doit entendre autre chose par les couleurs que

This was a grand conception, for which the philosophy of optics is under an obligation to the inventor greater perhaps than has been confessed. But Descartes's views in physics were not exact enough to admit of his turning such a conception to its full account. He seems to have had no idea of intermittent or elastic forces, and did not even endow either his filaments of air, or his æthereal globules interposed between them, with attractive or repulsive powers.

The genius of Hook, so comprehensive of clear physical notions, soon lent to this luminiferous æther the mechanical attribute which it needed, and added the notion of *vibratory pulses*,—a notion which was instantly reduced by Newton to the form most competent to account for the phænomena\*, and on which Huygens founded, and Young with his illustrious coadjutors have gone far to complete, the mathematical fabric of the undulatory theory of light.

So necessary indeed to any account of the phænomena of les différentes variétés qui arrivent en ces propensions.” (*Oeuvres*, tom. vii. p. 193). “J'admire que vous alléguiez les pages 4 et 5 afin de prouver que le mouvement des corps lumineux ne peut passer jusques à nos yeux, qu'il n'y passe quelque chose de matériel qui sorte de ces corps; car je ne fais en ces deux pages qu'expliquer la comparaison d'un aveugle, laquelle j'ai principalement apportée pour faire voir en quelle sorte *le mouvement peut passer sans le mobile*; et je ne crois pas que vous pensiez lorsque cet aveugle touche son chien de son bâton qu'il faille que ce chien passe tous le long de son bâton jusque à sa main, afin qu'il en sent les mouvements. Mais afin que je vous réponds *informé*, quand vous dites que le mouvement n'est jamais sans le mobile, *distinguo*; car il ne peut véritablement être sans quelque corps, mais il peut bien être transmis d'un corps en un autre, et ainsi passer des corps lumineux vers nos yeux par l'entremise d'un tiers, à savoir, comme je dis en la page 4, par l'entremise de l'air et des autres corps transparents, ou comme j'explique plus distinctement en la page 6, par l'entremise d'une matière fort subtile qui remplit les pores de ces corps et s'étend depuis les astres jusques à nous” (p. 240).

\* Phil. Trans., No. 88, p. 5088. An. 1672. “The most free and natural application of this hypothesis I take to be this: That the agitated parts of bodies, according to their several sizes, figures, and motions, do excite vibrations in the æther of various depths or sizes, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal sizes be separated from one another, the largest beget a sensation of a red colour, the least, or shortest, of a deep violet, and the intermediate ones of intermediate colours.”

light and colours did the admission of such a medium appear, that Wallis, who not only rejected the use which Huygens and others proposed to make of it in explaining the extraordinary height at which mercury, purged of air, may be suspended in a tube, but doubted even its possessing the properties of elasticity and weight, nevertheless did not scruple to say, "That there is in our air a body more subtle than the fumes and vapours mixed with it in our lower region *seems to be very certain*: but whether that subtle body be, as Dr. Garden seems to suppose, much heavier than our common air, I much doubt, and rather think it is not, not having hitherto had any cogent experiment either to prove it heavy or elastic; but it may, for aught I know, be void as well of weight as spring, and what is found of either in our common air may be attributed to the other mixtures in it\*."

There are few things more remarkable in scientific history than the manner in which Newton may be observed to have dealt with the conjectural part of philosophy. He never speaks of hypothetical speculation but in terms implying somewhat of disdain. And yet in all his works, from the announcement to the Royal Society of his first discoveries respecting light to the last revision of the Optics and Principia, an hypothesis of the highest generality holds a conspicuous place.

This apparent inconsistency is however easily explained: he doubtless was deeply impressed with the error into which his predecessor Descartes had fallen, in building a system of philosophy on superficial analogies and precarious conjectures, and looked with some dissatisfaction at the pretension of his contemporary Hook to set aside the inductive analysis of light, on the faith of a conjectural standard of his own. With Newton the imagining hypotheses was but as child's-play compared with the labour and importance of those severe and sure processes, inductive and deductive, to which he had devoted all the efforts of his mind. He held cheap the exercise of that great faculty of imagination from which the inexhaustible riches of his philosophical invention flowed with spontaneous facility. But though he laid no stress on what he called his "guesses," no man's mind seems ever to have

\* Phil. Trans. No. 171, p. 1002.

been more continually, as it were, *upon the guess*; and no one ever gave so eminent and instructive an example of steady persistence in that conjectural habit of mind. “To show,” says Newton, “that I do not take *gravity* for an essential property of bodies, I have added one question concerning its cause, choosing to propose it by way of question because I am not yet satisfied about it for want of experiments\*.” After having himself achieved by a vigorous induction the most extensive generalisation to which the human intellect has ever attained, he still saw, in a stronger light than any one, reasons for doubting whether the law at which he had arrived was so simple and conformable to the rest of nature as to preclude our tracing it to some more general cause. The ascertained rule of gravitation he used but as a stepping-stone on which he might safely tread in advancing towards the great end of philosophy,—the reduction of all that is implied in the terms space, force, and matter, to the closest relations and the fewest agencies: he regarded this great discovery with no more partiality than he did the more undeveloped principle of molecular cohesion, with respect to which, after stating his general conception of the force, he comes to this conclusion—“there are therefore *agents* in nature able to make the particles of bodies stick together by very strong attractions; and *it is the business of experimental philosophy to find them out*†.”

The term *attraction*, be it observed, was always employed by Newton in a provisional sense. “How these attractions may be performed,” he says, “I do not here consider: what I call *attraction* may be performed by *impulse*, or by some other means unknown to me; I use that word here to signify only in general *any force by which bodies tend towards each other*, whatsoever be the cause:” thus he was content to express, in any terms that lay at hand, the mathematical law of the force, whilst he kept the efficient cause in reserve, laying down for the order of investigation this rule—“We must learn from the phænomena of nature what bodies attract one another, and what are the laws and properties of the attraction, before we inquire the cause by which the attraction was performed‡.”

\* Advertisement to Optics, 1717.

† Optics, Book 3. Qu. 31.

‡ Ibid.

The cause of gravity, whatever it may be, he conceived must also lie at the foundation of all the other great classes of force which we observe, and till their laws and properties should have been learnt, he knew that it would be premature to attempt any deep inquiry into their causes. Nevertheless he let loose his fancy in more than one excursion into this wide field of speculation; and it is worth our while to mark the manner in which he surveyed it. For he possessed beyond other men that double power of mind which can adapt itself equally to the furthest and nearest limits of vision, and cast a glance as comprehensive over remote objects, as precise and penetrating into those that are within reach.

The widest of the generalisations to which the conjectures of Newton ascended were marked by a character far different from any which appears in the speculations of those who preceded him. Instead of loose or narrow analogies, in forming his ideas of the interior mechanism and materials of the universe, he clothed the phantoms of his philosophical vision with the most certain and general of the properties of matter: for the hooked atoms of Epicurus, the broken fragments, subtle powder, rounded globules, and feathery filaments of Descartes, he substituted the conception of particles embodying invariable powers of inertia, solidity, and hardness, with forces attractive or repulsive, which vary according to aggregation and distance. Of such particles, grouped in various modes and degrees of condensation, and variously moulded by the hand of the “Protoplasm,” he thought all material things might be imagined to consist,—by such both the stability of nature and the conservation of motion might be maintained,—and from such all the great classes of phænomena might be derived.

The general name which he gave to the simplest of these particles, collectively considered in a state of mutual repulsion, was *æther*—a term borrowed from Descartes, which Newton used to express the substance of one, or more, highly subtle and elastic fluids, capable of being combined and condensed, and taking, in different states of condensation, the forms of light and ordinary matter.

His *æther* was not a mathematical or mechanical abstrac-

tion, but a material substance, of the actual existence of which, certain otherwise uninterpretable phænomena, especially of light, heat, and electricity, had convinced him, and which he conceived of, as being “much of the same constitution with *air*, but far rarer, subtler, and more elastic”—“not of one uniform matter, but composed, partly of the main phlegmatic body of æther, partly of other various ætherial spirits, much after the manner that air is compounded of the phlegmatic body of air intermixt with various vapours and exhalations,”—one of these spirits being the electric, another the magnetic, a third the gravitating principle. The latter he figured to himself as “not of the main body of phlegmatic æther, but of something very thinly and subtilely diffused through it (perhaps of an unctuous, or gummy, tenacious and springy nature\*), and bearing much the same relation to aether which the *vital aërial spirit*, requisite for the conservation of flame and vital motions, does to *air* †.”

This was the first speculation of Newton respecting “the cause of the gravitating attraction of the earth.” “For if such an ætherial spirit,” he adds, “may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter, for the continual uses of nature adhering to the sides of those pores after the manner that vapours condense on the side of a vessel, the vast body of the earth, which may be every where to the very centre in perpetual working, may continually condense so much of this spirit as to cause it from above to descend with great celerity for a supply: in which descent it may bear down with it the bodies it pervades with force proportional to the superficies of all their parts it acts upon, nature making a circulation by the slow ascent of so much matter out of the bowels of the earth in an aërial form, which for a time constitutes the atmosphere, but being continually buoyed up by the new air, exhalations, and vapours rising under, at

\* Such expressions as these, used only in the earliest of Newton's speculations, appear to be in the style of the Epicurean school; but his meaning, as is evident from the variety of the terms which he uses, was only to describe in *popular language*, attractive and repulsive force.

† Registry Book of the Royal Society, vol. v. from 1675 to 1679, p. 69.

length (some part of the vapours which return in rain excepted) vanishes again into the ætherial spaces, and there perhaps in time relents and is attenuated into its first principles. For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fluid, subtile out of gross, and gross out of subtile, some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding further from him: and they that will may also suppose that this spirit affords, or carries with it, the solar fuel and material principle of light, and that the vast ætherial spaces between us and the stars are for a sufficient depository for this food of the sun and planets\*."

How far in a geometrical and mechanical point of view a supposition which presents to us the problem of an uniform central loss of force in a sphere of "*tenacious or springy*" fluid, urged by a constant pressure, and drawing down or impelling the bodies that float in it with a force proportional to the number of their ultimate particles, can have been contemplated as tending to satisfy the conditions of the law of gravity, I leave to mathematicians to judge. This supposition preceded the public announcement of the law by ten years; but Newton has himself stated that he had deduced that law from Kepler's some twenty years before he published it†.

He soon, however, in a letter to Boyle in 1678, abandoned this form of hypothesis for one in which he supposes the æther no longer a gradually absorbed, centripetal, atmosphere, but a *stationary* fluid, "which consists of parts, differing from one another in *subtilty* by indefinite degrees," so arranged by the force with which the *pores of matter* repel the *ætherial particles* in proportion to their *magnitude*, "that from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the æther is insensibly finer and finer;" and in an ætherial atmosphere so constituted he holds

\* Registry Book of the Royal Society, vol. v. from 1675 to 1679, p. 70.

† Letter of Newton to Halley, 1686.

that bodies would be propelled towards each other by the assumed greater repulsion of the larger particles of æther from their pores. In this letter he made a comprehensive conjectural effort to reduce the whole system of the laws of nature, whether bearing the aspect of impulse or attraction, under the dominion of *two kinds of repulsive force*, the one of mutual repulsion between the particles of æther, the other of repulsion between the particles of æther and those of ordinary matter.

In the edition of his *Optics* which he printed nearly forty years afterwards, in 1717, he deliberately delivered, when in full possession of the laws of gravity, another hypothesis on this subject, taking for his fundamental assumption this fact presumed from the phænomena of light, that a subtle and elastic fluid, within bodies and without them, increases in density from their centre indefinitely into space, and merely representing the force by which they gravitate as *repulsive*. Further he has not explained himself; and it may perhaps be inferred from his subsequently omitting in an edition of the *Principia* the mention of *gravity*, when he enumerates, at the end of that work, the other phænomena of molecular attraction and cohesion, electricity, light, heat, muscular motion, and nervous sensation, which he attributes to the force of “a very subtle spirit,” pervading and lurking in dense bodies, but not yet sufficiently manifested by experiments,—that he was dissatisfied with his own conceptions of its gravific action, and had never reduced them into a mathematical form. Thus much however it may be worth while to remark, as deserving perhaps the attention of those who may follow Vince and Playfair in discussing the possible sufficiency of Newton’s hypothesis—that in the *Optics* he alleges reasons for supposing the *elastic force* of ætherial particles to be *inversely proportional* to their *magnitude*\*. This leaves ground to believe that with the superposition of a *density* increasing with the distance he may have combined his former conjecture of an increasing *magnitude* of the particles, and so far an elasticity proportionably diminished; which gives latitude at least to the hypothesis, as making the mutual repulsion of the particles, at different distances from the centre, depend on more elements than one.

\* *Optics*, ed. 4. book iii. p. 326.

But the knowledge of the experimental laws of molecular force was not sufficiently advanced to justify any serious attempt at mathematical theory, either on this subject or any other connected with them ; nor did he offer these hypotheses as more than cursory hints, and specimens of a generalising and simplifying spirit of conjecture, so far illustrating nature, as they embraced, and embodied, real facts and accurate conceptions of phænomena.

The subjects on which in this point of view the above-mentioned hypothetical views, taken conjointly with the *queries* in the Optics, threw the most important light, were the phænomena of colours, and of chemistry. I shall confine myself to his speculations on the latter subject, which lead directly to the question at issue—namely, what were the ideas of philosophers before the time of Black respecting the nature of air ;—was the *unity* of the aërial element any part of their belief?

Most remarkable, among the *divinations* of Newton, is his *introduction* of the doctrine of chemical affinity in the optical *queries*, where he connects the phænomena of chemistry with those of electricity, as both due to *molecular forces acting at insensible distances*. He enumerates electricity among those “attractions which reach to sensible distances, and so have been observed by vulgar eyes ;” he then suggests, that “there may be *others* which reach to so small distances as hitherto escape observation,” and adding that “perhaps *electrical attraction may reach to such small distances, even without being excited by friction*,” goes on to couple it with the phænomena of *chemical affinity*, as produced by the same species of force. What is this, if it be well weighed, but the principle of all that *éxperience* has since brought to light in respect to galvanic and electro-chemical forces ? here was the prophet’s eye, anticipating the progress of science and the actual indications of the kind of force which he surmised.

That which follows on the point of chemical affinity itself is equally remarkable. For observe how, guided in this instance by the few obscure phænomena before him, he deals with the molecules which represent this peculiar form of attraction : they are not mere elementary molecules, nor molecules of

\* Optics, book iii. p. 351.

equal magnitude, but *compound particles whose force of affinity is in the inverse ratio of their composition*—“the smallest particles cohering by the strongest attractions, and composing bigger particles of weaker virtue, and many of these cohering, and composing bigger particles whose virtue is still weaker, and so on, for divers successions, until the progression end in the biggest particles on which the operations of chemistry, and the colours of natural bodies depend, and which by cohering compose bodies of a sensible magnitude\*.” Have we not, in this conception of chemical affinity as depending on the *successive addition of units of force*, the principle of *multiple proportions*, of which the experimental demonstration was reserved for Dalton, whose first views of that important induction were suggested perhaps by these very conjectures of Newton?

In other respects the theory of affinities is hardly laid down by him with more distinctness in this mature work, than in his younger speculations, in the earliest of which he applied it, as an universal property of bodies, to supposed ætherial fluids, and in the next to the factitious airs then recently discovered.

The chemist who remembers the modern observation, that gases (including that *vital aërial spirit* to which Newton compared his æther) are powerfully condensed in the pores of charcoal, on the surface of metals, and in the interior of a ball of spongy platina, cannot fail to be struck with the singular anticipation which the *first* of Newton's *hypothetical* writings displays, of a close connexion between molecular attractions and chemical changes, and a subjection of the most elastic of bodies to both these forces in common. Nor will his admiration be diminished, when he finds the theory of elective and mediating affinities *first* broached for such a purpose as to explain the dark phænomena of muscular motion, and the material means through which the soul acts on the body, by the supposition of *relative degrees of sociableness and unsociableness* between the brain and muscles on the one hand, and on the other, a conjectural array of ætherial fluids imagined to be even

\* Optics, book iii. p. 370.

rarer and more elastic than the most subtle and repulsive air\*.

After this, we are not astonished to find the same master mind, in its *second* survey, so laying down the theoretical map of gaseous chemistry, that in truth the chemists who followed, down to the *æra* of Higgins, Dalton, and Gay-Lussac, did little more than work out by experiment the principles which Newton had assumed.

The application of chemical principles to ætherial matter is contained in a letter to Oldenburg, from which I have already given some quotations, read before the Royal Society in Dec. 1675. This elaborate communication, strange to say, has never been printed, except in the ponderous and seldom opened volumes of Birch's history of that Society, and consequently is scarcely known, even in our own country, to men of science, otherwise than by a few extracts from that part of it which relates to light, published in the Philosophical Transactions by Dr. Young.

The theory of gases, as communicated to Boyle in 1678, you will find in Birch's life of that philosopher, or in Newton's collective works. In his letter to Boyle, after supposing certain atmospheres of æther to surround the particles of bodies, and describing a pressure of elastic forces, which varying with the distance produces cohesion at small distances, and repulsion at greater, he deduces among other consequences this—"that the particles of vapours, exhalations, and air, do stand at a distance from one another, and recede as far from one another as the pressure of the incumbent atmosphere will let them: for I conceive," he says, "the confused mass of vapour, air, and exhalations, which we call the atmosphere, to be nothing else but the particles of all sorts of bodies of which the earth consists, separated from one another and kept at a distance by the said principle."

He then proceeds to distinguish the *three* different ways which nature has of "transmuting gross compact substances into ærial ones"—vaporisation—volatility—and the liberation of fixed air, and to propose a theory to explain the differences. From the hypothesis, to which I before alluded, of

\* Letter to Oldenburg, Registry of the Royal Society, vol. v.

a double repulsive force, producing unequal degrees of æthereal pressure, he deduces different spheres of cohesion and repulsion for different bodies, and their particles, in proportion to their density and size: small particles are easily detached, and easily condensed; and this is the condition of volatile substances, and of liquids—"when the particles of a body are very small, as I suppose," he says, "those of water are, the action of heat may be sufficient to shake them asunder;" and "as fast as the motion of heat can shake them off, those particles, by the said principle, will float up and down at a distance from one another, and from the particles of air, and make that substance we call vapour." "But if the particles be much larger, they then require the greater force of *dissolving menstruums* to separate them." Thus he comes to the chief object of this letter, which was to illustrate the theory of gases—of the substances, that is, then recently discovered to be more *durably fixed*, and more *durably aërial*, than vapours or volatile effluvia. For this purpose, having assumed that the essence of such substances is, that their constituent particles are relatively larger and denser, and therefore, by hypothesis, more elastic than others in the *aërial*, and more cohesive in the *fixed* condition, he brings in the doctrine of chemical affinities, elective and mediate, to liberate them from their close state of cohesion, and force them out of the proximate sphere of compression into the remoter one of repulsion. And thus, as subsidiary to the play of a philosophical fancy, were those great principles laid down, which experience has subsequently verified, and on which the whole fabric of the chemistry of solids, liquids, and gases, has been built.

In these views the new discovery of the various permanence and condensability of the gases has a conspicuous place: "On the same difference of size," he says, "may depend the more or less permanency of aërial substances in their state of rarefaction." "This may be the reason why the small particles of vapours come easily together and are reduced back into water, unless the heat which keeps them in agitation be so great as to dissipate them as fast as they come together, but the grosser particles of exhalations raised by fermentation keep their aërial form more obstinately, because the aether within is

rarer. Nor does the size only, but the density, of the particles also conduce to the permanency of aërial substances: for the excess of density of the æther *without* such particles above that of the æther *within* them is still greater: which has made me sometimes think that the true permanent air may be of a *metallic* original, the particles of no substances being more dense than those of metals. This I think is also favoured by experience: for I remember I once read in the Philosophical Transactions how M. Huygens at Paris found, that the air made by dissolving salt of tartar would in two or three days' time condense and fall down again; but the air made by dissolving a metal continued without condensing or relenting in the least. If you consider then how by the continual fermentations made in the bowels of the earth there are aërial substances raised out of all kinds of bodies, all which together make the atmosphere, you will not perhaps think it absurd, that the most permanent part of the atmosphere, which is the true air, should be constituted of *these*; especially since they are the heaviest of all others, and so must subside to the lower parts of the atmosphere and float upon the surface of the earth, and buoy up the lighter exhalations and vapours to float in greatest plenty above them. Thus I say it ought to be with the metallic exhalations raised in the bowels of the earth by the action of acid menstruums; and thus it is with the true permanent air."

These extracts show that Newton considered the hydrogen gas which Boyle had obtained from iron, and the nitrous gas which Huygens had obtained from copper, as consisting of the ultimate particles of the iron and copper themselves, brought into a state of aërial elasticity; and further, that apprehending his ætherial hypothesis to be thus strengthened by experimental facts, he proceeded to generalise so boldly, as to conclude that the whole body of the inferior atmosphere may be constituted of various metallic substances, and that the power and persistence of elastic force in different kinds of air may be proportionate to the size and density of their chemical elements.

This supposition, that the most permanent airs are of a metallic origin and nature, representing hydrogen for instance

as *ferreous gas*, was set aside by the experiments of Cavendish, which proved that the gas from *iron* is identical with the gas from *zinc*, in specific gravity, in explosive power, in the quantity in which it combines with oxygen, and in the result of the combination: they went also, as far as our experiments reach, to invalidate the general supposition that the repulsive force of the particles of matter is in proportion to their weight and density; since they proved that hydrogen is, both in its *elastic* and in its *fixed* state, the lightest of bodies; unless indeed its high refractive power should be thought a stronger argument for the density, than its low combining weight for the lightness, of its molecules.

Newton seems not to have been aware, that the facts of the condensation of one gas and permanence of another, the observation of which he here ascribes to Huygens, had been established some ten years before by experiments instituted at the Royal Society, in which his correspondent Boyle had assisted—a circumstance however which was notified to the public when Huygens's paper was printed\*. The experiments themselves having, I think, never been published, the interest which we equally take in tracing back the history of science, the curiosity of the experiments, and the celebrity of the experimenters, prompt me to give you some extracts on this subject from the journals of the Society.

\* An account of Huygens's experiments was printed at Paris in 1674, and appears in the Philosophical Transactions, No. 119, dated November 22, 1675, under the title of—"some experiments made in the air-pump by M. Papin directed by M. Hugens." The following extract contains the facts to which Newton referred:—"The experimenter being desirous to see whether these ebullitions did make *new air*, put in the recipient a gage, and observed that when the liquors were mingled, the water in the gage rose very nimbly to the top of the gage; and drawing out the new air he made the gage-water subside again; and by this means it was seen, that all these kinds of ebullition make an air which expands itself like common air. Yet here is something that seems to be very remarkable, which is, that the air made by these ebullitions is not of the same nature: for it has been found experimentally, that the air formed by the mixture of *aqua-fortis* and copper remains always *air*, and always keeps up the water in the glass; but on the contrary, the air which has been made by the mixture of oil of tartar and oil of vitriol is almost all destroyed of itself, in the space of twenty-four hours. All these ebullitions hitherto spoken of are greater in *vacuo* than in the open air; but with lime it is not so."

From these it appears that on January the 4th, 1664, a year before the publication of the *Micrographia*, Hook exhibited to the Society “experiments to show that air is the universal dissolvent of *sulphureous* [combustible] bodies, and that this dissolution is fire, adding that this was done by a nitrous substance inherent in, and mixed with, the air.”

Here was the first distinct conception, and evidence, of the composition of the atmosphere. The French physician Rey had before proved that air enters into fixed combination with solid matter: his proof rested on a capital observation which he quotes from the *Basilica Antimonii* of Hamerus Poppius: this chemist, “placing,” says Rey, “a burning glass in the sun’s rays, directed their focus on the apex of a cone of antimony, till the whole becomes white, when the calcination is complete. It is a wonderful thing, Poppius added, that although in this calcination the antimony loses much of its substance by the vapours and fumes which exhale copiously, yet so it is, its weight increases instead of diminishing\*.” The philosophical acumen of Rey seized on the truth unequivocally shown in this simplified form of calcination, in which he discerned the presence of but two ponderables; and he concluded,—1. That the increase of weight arose from the air being solidified in the antimony; 2. That the two substances combined to a definite degree of saturation; 3. That the increase of weight observed in other metals, whether by calcination or simple exposure to the air, is due to the same cause—conclusions which, if their publicity had been equal to their value, would doubtless have been recorded, for the early and distinct enunciation which they contain not only of an important though as yet unanalysed fact, but of true chemical principles, as the first step in this branch of science, in consequence as well as time†.

\* *Essay 25.*

† Rey’s Essays were first published in 1630. They contain, besides the speculation here mentioned, a just correction of the view which *the schools* had taken of a fact affirmed in the Physics of Aristotle—that a blown bladder is heavier than an empty one. Rey showed that this is true only if the bladder be blown to such a degree as to *compress* the air, and that the fact, so stated, is a real proof that the air has *absolute weight*. This is perhaps the first correct *published* statement of the weight of air, as an experimental fact. It is evident however from a letter of Baliani, quoted by Venturi, that Galileo had not only taught the same doctrine, but made his experiments on the specific gravity of the air before 1630.

But Rey, whilst he recognised the ponderable and combining qualities of air, considered it with the other philosophers of his day, as an element simple in essence, though mutable in form: and the first scientific question of the accuracy of this supposition was raised by Boyle in 1654. In the same Essay in which his discovery of the factitious airs was announced, he quoted from Paracelsus the following remarkable passage:—"As the stomach converts meat, and makes part of it useful to the body, rejecting the other part, so *the lungs consume part of the air, proscribing the rest;*" and having observed upon it, that "though this opinion is not, as some of the same author's, absurd, it should not be barely asserted, but explicated and proved," proceeded to relate, that "that deservedly famous mechanician and chemist Cornelius Drebell contrived for the late learned king James a vessel to go under water," and that on inquiry of Drebell's surviving relatives into the principle of his contrivance, it appeared, that "he conceived it to be not the whole body of the air, but a certain quintessence or spiritual part of it that makes it fit for respiration, which being spent, the remaining grosser body, or carcase, if I may so call it, of the air, is unable to cherish the vital flame residing in the heart; so that, for aught I could gather," says Boyle, "besides the mechanical contrivance of his vessel, he had a chemical liquor which he accounted the chief secret of his navigation; for when from time to time he perceived that the finer and purer part of the air was consumed, or overclogged by the steams and respiration of those that went in his ship, he would, by unstopping a vessel full of this liquor speedily restore to the troubled air such a proportion of vital parts, as would make it again for a good while fit for respiration\*."

The experiments which Hook exhibited to the Royal Society in 1664 afforded, it must be allowed, but precarious grounds for the theory of the composition of the atmosphere which his sagacity advanced: he showed that in vessels containing a limited quantity of air, combustibles burn and waste for a limited time; and change its quality so that it is no

\* New Exp. Physico-mechanical.

longer capable of supporting combustion ; and he showed that they undergo no loss of substance when heated without air : he took some live coals and put them under a glass vessel—“ whereupon the said cole in a little time went out ; but being then taken out, and exposed to the free air, recovered its burning : ” sulphur in like manner would not burn when “ hermetically sealed,” and charcoal heated without air—“ was not sensibly diminished ; ” he added—“ that a combustible substance kept red-hot, yea in a fire as hot as to melt copper, would not waste, but as soon as fresh air was admitted did burn away and consume.” Boyle proposed that trial should be made whether the extinguished combustible could be re-lighted by the burning-glass, or by red-hot iron ; and it was found that it could not be rekindled without the admission of air.

Yet nothing can be more accurate than the theoretical account of combustion given in the *Micrographia*, where, laying down the principle that “ the different volatility, or fixedness, of the parts of bodies seems to consist only in this, that the one is of a texture, or has component parts, which will be easily rarefied into the form of air, and that the other hath such as will not without much ado be brought to such a constitution,” Hook states that “ in the dissolution of *sulphureous* [combustible] bodies, by a substance inherent in, and mixed with, the air, which is like, if not the *very same* with, that fixed in saltpetre, a certain part of the bodies is united and mixed, or *dissolved and turned into*, the air, and made to fly up and down with it, in the same manner as a metalline or other body, dissolved into any *menstruum*, doth follow the motions and progress of that menstruum till it be precipitated.”

Even the fact, afterwards proved by Cavendish, of the *density* of the gaseous product of this *dissolution*, was predicted by Hook ; for in an experiment—“ to prove that the substance of a candle or lamp is dissolved by the air, and the greatest part thereof reduced into *a fluid in the form of air*, ”—he observes, that “ the reason why this mixed body, which *certainly is otherwise heavier than the air*, and so ought to descend, doth notwithstanding ascend, is from the extraordinary rarefaction of the same by the nearness and centrality of the flame and

heat, whereby it is made much lighter than the ambient air\*."

The *production* of volatile salts in combustion, by an analogous process of combination, seems likewise to have been apprehended by him, where he represents "other parts of the combustible," not capable of the aërial form, as nevertheless so "mixing and *uniting* with the parts of the air," as "to

\* "Experiment to prove that the substance of a candle or lamp is dissolved by the air, and the greatest part thereof reduced into a fluid in the form of air—showed the Royal Society 22-29 Feb 1671-2."—Registry of the Royal Society.

"I took a large concave reflecting glass, or a large convex refracting one, and so placed it in respect to my eye that a candle, set at a certain distance beyond the refracting glass, or between the eye and the superficies of the reflecting glass, enlightened the whole area of the said glasses in respect of the eye. Then continuing to keep the eye in that place where the area of the glasses appeared to be wholly filled with the flame of the candle, I caused another candle to be placed very near the said glasses, between the eye and the glass, or beyond also if I used the refracting glass, then looking steadfastly at the flame of the last candle, it was very plain to be perceived, that the flame thereof was encompassed with a stream of liquor, which seemed to issue out of the wick, and to ascend up in a continued current or *jet d'eau*, and *to keep itself entire and unmixed with the ambient air*, notwithstanding that it was a considerable way carried above the aforesaid flame. It was further very plain that the said distinct fluid did make several turnings, whirlings or vertices in the ambient air as it ascended higher and higher, and by degrees mixed itself with the ambient air. "T was yet further observable that the shining flame was placed in the middle of this *jet d'eau* at the lower end thereof, but that it did not ascend proportionally in height to the height of the *jet d'eau*, that where the tip of the flame ended, there ascended up a small line of an opacous body or smoke, which to a good height above the flame kept the middle of the stream. The manifestation of these phænomena was from the differing refractions of the body of the *jet d'eau* from that of the ambient air; for the flame of the first candle being but small and placed at a considerable distance from the refracting or reflecting glass, the smallest variation in the refraction of the medium between the first candle and the eye caused the darkness to intermix with the light, so as to exhibit the appearance of the heterogeneous *jet d'eau*. This *jet d'eau* I suppose to be nothing else but the mixture of the air with the parts of the candle which are dissolved into it in the flame. The reason why *this mixed body, which certainly is otherwise heavier than the air*, and so ought to descend, doth notwithstanding ascend with great swiftness, is first from the ascent of the flame in the middle, and next from the extraordinary rarefaction of the same by the nearness and centrality of the flame and heat, whereby it is made much lighter than the ambient air."

make a coagulum or precipitation, as one may call it, which is separated from the air," but being light and volatile is carried up by its motion, till the agitation that kept it rarefied ceases, and it condenses into "a certain salt which may be extracted out of soot :" and the view thus expressed appears from the Registry to have been corroborated at one of these sessions by Boyle, who observed that "vegetables reduced in the open air yield store of volatile salt like that of hartshorn and other animal bodies, whereas in common distillations he had not found them to yield a grain."

Hook produced evidence also before the Society of that sameness of effect, by which he identified the particular ingredient in the air that supports combustion with one of the fixed constituents of nitre. To this purport he "made an experiment with charcoal enclosed in a glass, to which nitre being put, and the hole suddenly stopped up, the fire revived, although no fresh air could get in,"—and another "of gunpowder burning without air."

It is curious to remark that a similar experiment was made some fifty years before by the Cabballist and Rosicrucian antagonist of Kepler and Mersenne, Fludd; who in proof "that the substance of saltpetre is nothing else but *air* congealed by cold\*", relates that he filled an egg with it, mixed with sulphur and quick lime, and closing the aperture with wax placed the egg under water, where it exploded. Fludd also burnt a candle in a glass vessel over water, and observed that it raised the water in proportion to the quantity of air, enclosed in the vessel, which was consumed and burnt; for "air," he adds, "nourishes fire, and in nourishing it, is con-

\* "Videmus salis petrae substantiam nihil aliud esse quam *aërem* frigore congelatuni, cui si accedit sulphuris aliqua portio, licet exigua, admodum strepitum ingentem edit, fulguraque artificialia emittit."—Utriusque Cosmi Historia, vol. i. tract. 1. lib. 7. cap. 6. De fulmine et tonitru, 1617. "In 2dâ demonstratione, candela in fundo vasis alicuius aquâ repleti affigitur, cuius flamma per orificium phialæ ingrediens, depresso ejus orificio ad angulos rectos cum candelâ in vasis aquæ, sursum attrahet tantam aquæ proportionem quantam aëris in phialâ inclusi consumpsit; aër enim nutrit, ignem, et nutriendo consumit; ac ne vacuum admittatur, aqua, hoc est tertium elementum, locum possidet aëris comesti."—Ibid. tract. 2. part. 1. lib. 3. Reg. 6.

sumed." This sounds like the truth which Hook announced: but Fludd had no distinct idea of *the weight* of air, or of the great principle which led that philosopher to predict, and observe, *ponderable* products from its consumption.

Boyle supported Hook's views by "affirming that gunpowder burns very well in a receiver out of which the air has been extracted," and he afterwards took the pains to experiment with nitre compounded of nitric acid and potash out of contact with the atmosphere "*in vacuo Boyleano*," for the sake of "removing the suspicion that it does not burn without air being supplied by the numerous eruptions of the aërial particles *intercepted* by those that by their coalition make up the nitrous corpuscles\*." Boyle also remarked at this discussion that "*tin* mixed with nitre will kindle it;" to which Hook added, that filings of *iron* will do the same. This remark was justly deemed of such importance, that the Society ordered the experiment to be tried; and it was found that "filings of tin being cast on nitre, over a fire, made it flame; though it be not known," adds the writer of the Minutes†, "that *sulphur* was ever extracted out of tin; which seems to infer that there are bodies *combustible* which are not *sulphureous*."

The only verification, in the Registry, of the intimation given in the Micrographia that the same principle in the air which supports combustion is concerned "in respiration and the preservation of life," consists in an experiment suggested by Dr. Ent, in which a bird was enclosed with a chaffer of live coals in a receiver; the Society observed the extinction of the fire to be followed by failure of vitality in the bird, which revived on the readmission of air.

After these inquiries Dr. Wilkins proposed (on the 8th of March 1664)‡ "that the following experiment (of Dr. Wren's suggestion) might be made, viz. to put a fermenting liquor in a glass ball to which a stop-cock should be fitted, and to tie a bladder about the top of the stop-cock, by which means a certain air generated by the fermenting liquor would pass into the bladder, and upon the turning of the stop-cock be kept there in the form of air without relapsing into water. This,

\* New Experiments touching Flame and Air. † Oldenburg.

‡ Reg. R. S. 1664, and Birch's Hist. R. S., vol. ii. p. 21.

or the like, to be tried at the next meeting. Mr. Hook mentioned several liquors that by their working upon one another would generate *an air*; viz. oil of tartar and vitriol, spirit of wine and turpentine, &c. Colonel Blunt added that oysters pounded and put into wine would make it ferment."

"On the 15th of March, the experiment of generating air was made in this manner. There was taken a common glass phial with two pipes, and some pounded oyster-shells and aquafortis; and as soon as the aquafortis was by one of the pipes poured in upon the powder, and the hole stopped with a piece of hard cement, the ebullition caused by the corrosion of the shells by the aquafortis did in a very little time blow up the bladder (tied on the other pipe) so as to swell it with air, very plump; which expansion remained till the rising of the Society, when the vessel in that posture was locked up in the box of the watch, to remain there until the next assembly." Dr. Wren made use of this experiment—"to explicate the motion of the muscles by explosion." "There was also taken a bottle containing strong ale that had been bottled awhile; and over the bottle's mouth was tied an ox-bladder out of which the air was squeezed; after which by loosening the cork by degrees the air was blown out into the bladder by the expansion of the fermenting liquor within, and the bladder was almost half-filled with an aërial spirit generated by the working liquor." Mr. Boyle, bearing perhaps in mind his anecdote of Drebell's submarine vessel, suggested that the experiment was capable of improvement for the producing of air under water, and mentioned coral, or oyster-shells, and distilled vinegar, as wholesome substances for that purpose: he moved that an animal might be put into the receiver of his engine and the air exhausted till the creature grew sickly, and that then some new air might be produced in the receiver by a contrivance of making distilled vinegar work upon coral, to see whether by this means the animal could be revived. Dr. Wilkins moved that at the next meeting the air generated by the mixture of aquafortis and the pounded oyster-shells might be blown into a dog's or cat's mouth, to see what would be the effect thereof."

"On the 22nd of March there were two experiments made

for the finding out a way to breathe under water, useful for divers. The first was made by putting a bird into a rarefying engine, and with it a glass bottle with distilled vinegar and pounded oyster-shells, which whilst the vinegar is dissolving them affords a stream supposed to be *a kind of new air* fit for respiration. The bottle was also close stopped with a cork, so ordered that by pulling the stop-cock placed on the top of the receiver the cork might, by turning it, be pulled out without admitting an ingress of the external air into the receiver at all: then the receiver being accurately cemented to the engine the air was pumped out; whereupon the bird grew sick, and when he was thought near dying, the bottle was unstopped, that the streams and supposed air that had been shut up in it during the operation might have liberty to expand themselves in the receiver for the refreshing and recovering of the animal: but here it succeeded not; in so much that though the bird was taken out of the receiver and exposed to the fresh air, yet it recovered not."

"The other experiment was made with a *kitling* after the manner of the former, only that instead of distilled vinegar was employed aquafortis, whereof the success was, that the air being drawn out till the cat had done struggling, and was upon the point of expiration, and the bottle being unstopped to emit the streams and supposed air into the receiver, the cat did soon begin to recover, whereupon the animal had fresh air given it, which was again exhausted, to see whether it would revive of itself, without any nitrous exhalation; but after this exhaustion the cat appeared to be dying, whereupon she was after a little while taken out into the open air wherein she revived again."

"It was also moved that a standard might be used to know what quantity of air was generated."

"The glass phial with the swelled bladder, experimented upon at the last meeting and shut up till this day, was produced, and the bladder found evidently shrunk. Ordered to be tried next day with a glass phial whelmed under water, thereby to gather all the bubbles of the air generated by the corrosion."

"On being inquired how it was known that that which was

supposed to be air produced by the dissolving of pounded oyster-shells by spirit of nitre, or distilled vinegar, or aquafortis, was true air, and answer being made by the President\* —that *a body rarefied by heat, and condensed by cold, was air*, the bladder was put to the fire where it expanded again as much as formerly and being removed from thence became somewhat flaccid again.”

“ It being moved that it might be tried whether the streams produced by the operation of distilled vinegar upon the powder of oyster-shells were convenient for respiration, the trial was made, and the bottle wherein that dissolution was performed carried about to the company for every one to smell to it, and it was found by most of the company incommodious, as it was undiluted.”

“ It being moved by Mr. Hook that the air-boxes contrived for diving might be tried by the persons bespoke by Mr. Pepys for diving, it was ordered that this diver should be sent to Mr. Hook to be instructed by him touching the use of the said boxes under water.”

“ On the 29th of March an experiment was made for the generating of air by putting aquafortis and the powder of oyster-shells in a small glass phial under water, and whelming a large glass filled with water over it to receive the steam to be generated by the corrosion: the success whereof was that the whelmed glass was filled about  $\frac{1}{4}$ th full with an aerial substance—ordered to be set by till the next meeting.”

“ It was moved that a way might be thought on, of producing an air that might be useful to respire.”

“ On the 12th of April Mr. Boyle proposed [inter alia] to try whether the eggs of silk worms and snails would be hatched, as also whether seeds would germinate and thrive, all, in an exhausted receiver.”

“ Dr. Goddard affirmed that plants live as much upon air as the earth.”

“ Mr. Hook, being called upon to give an account of one of the last days experiments touching the air generated by aquafortis and the powder of oyster-shells, reported that the greatest part of it was returned into liquor.”

\* Lord Brouncker.

“ The same was ordered to make, the next day, the experiment of generating air with bottled ale, supposed to be wholesome to breathe in, which the air hitherto generated is not\*.”

On June the 14th “ an account was given of an experiment of the growth of water-cresses in a receiver.” Having been kept for a week in an exhausted receiver they showed no growth ; the air being admitted “ they grew in the same time two or three inches†.”

These experiments remained unprinted : but a more complete discussion of the same subjects not long afterwards appeared. In 1668, at the early age of 23, Mayow, adopting the theory of Hook, published a tract in which “ he delivered his thoughts of the use of respiration, waving those opinions that would have it serve either to cool the heart, or to make the blood pass through the lungs out of the right ventricle of the heart into the left, or to reduce the thicker venal blood into thinner and finer parts, and affirming that there is *something in the air* absolutely necessary to life, *which is conveyed into the blood*, which whatever it be being exhausted the rest of the air is made useless, and no more fit for respiration ; where yet he doth not exclude this use, that, together with the expelled air, the vapours also steaming out of the blood are thrown out. And inquiring what that may be in the air so necessary to life he conjectures that it is the more subtle and nitrous particles with which the air abounds which are communicated to the blood through the lungs, and this *aërial nitre* he makes so necessary to all life, that even plants themselves do not grow in earth deprived thereof‡.”

In 1673-74 he gave a fuller account of his opinions in another treatise§, the views contained in which exhibit one of

\* On the 24th of May in this year (1664) the following record is entered : “ The king had been pleased himself to make the observation (on the variation of the needle) at Whitehall, and had found no variation at all, the needle standing in the meridian.”

† I find that many of the extracts here given from the registry have been printed in Birch’s History of the Royal Society.

‡ “ An account of two books—Tractatus duo, prior de Respiratione, a Joh. Mayow, Oxon. 1668.” Phil. Trans. No. 41. p. 833.

§ Tract 5. *Med. Phys.* Imprim. Jul. 17, 1673.

the finest examples extant of the success with which a man of philosophical genius, having seized a true principle, may deduce from the observation of a few facts distinctly apprehended a whole train of real and important consequences, long before the principle itself can be deemed to have been proved by demonstrative experiments.

In reproducing the theory of the Micrographia (with new deductions and new evidence), he took no care to give the original author of it the credit which was due; and in his own turn is passed unmentioned by Lavoisier, who did not distinguish the chief precursor of his own discoveries from the rest of his chemical predecessors, on whom he pronounces this general censure—that “they all allowed themselves to be carried away by the spirit of their age, which contented itself with assertions without proofs, or at least often regarded very slight probabilities as such\*,”—a censure which it is but just to qualify by the reflection, that in experimental philosophy solid proofs are not to be discovered without the preliminary of happy conjectures.

In this tract Mayow expressly says, “Though the particles of air are very minute, *and are vulgarly taken for an element of the greatest simplicity*, it appears to me necessary to judge them to be a *compound*†;” and he adds,—“it is manifest that the air is deprived of its elastic force by the respiration of animals much in the same manner as by the deflagration of flame.” The latter assertion he made good by experiment: he not only observed, but measured, *the amount of elastic force lost in both these cases*; and he proved that animals, when confined in air which has been already diminished by combustion, *survive but half the time* that they would have lived in an equal volume of common air‡. But he advanced little

\* *Traité de Chimie. Discours préliminaire*, tome i. p. 16.

† *Tract. de parte aëreâ igneâque Spiritus Nitri*, cap. 7. p. 114.

‡ *Tract. de parte aëreâ igneâque Spiritus Nitri*, cap. 7. p. 101. “Comperi aërem per lucernæ deflagrationem in spatiu ex parte circiter tricesimâ minus quam antea reductum esse. Postquam fumi lucernæ deflagrantis, quibus encurbita predicta repleta est, prorsus evanuerunt, vitrumque intus æque ac prius pellucidum evasit, conatus sum secunda vice lucernam in eadem accendere, radios solares in aliam camphoræ portionem, in vitro eo

beyond his predecessor in demonstrating the air to be a compound. "It is not to be supposed," he says, "that that aërial supporter of combustion is *the whole air*, but only *a part of it*, which is more active and subtle than the rest; since a light enclosed under a glass expires, even whilst the vessel still contains abundance of air: for we cannot believe that the particles of air which *were* in the said glass can be *annihilated*, nor yet *dissipated*; since they cannot pass through the glass." For this reasoning, though probable, is not conclusive; since it was certainly possible that the enclosed air might have been diminished by condensation instead of abstraction, and have become unfit to burn and to be breathed by a total vitiation, instead of a partial loss.

Yet, after all, Mayow's reasoning appears to advantage by the side of Priestley's, or Scheele's, even when in the progress of experiment the *nitro-aërial spirit*, or fire-air, had been actually divorced from "*its consort*," and when the latter great chemist had approached a complete analysis of the atmosphæri pars suspensam, uti prius conjiciendo. Verùm experimentum non successit, indicio satis manifesto aërem istum per lucernæ deflagrationem particulis igneo-aëriis deprivatum esse, ita ut idem ad flammam denuo sustinendam prorsus inidoneus sit."—Ibid. pp. 105, 106. "Et quidem experimento cum animalibus variis facto compertum habeo aërem in spatiū ex parte circiter decimâ quartâ minus quam anteā per animalium respirationem reductum esse." . . . "Ex dictis certè constat animalia respirando particulas quasdam vitales, *ea*que *elasticas*, ab aëre exhaūrire, ut minimè jam dubitandum sit aëriū aliquid ad vitam prorsus necessarium sanguinem animalium respirationis ope ingredi."—Ibid. p. 107. "Ex quibus manifestum est aërem per animalium respirationem haud multo secus ac per flaminæ deflagrationem vi suâ elasticâ deprivari, et utique credendum est animalia ignemque particulas ejusdem generis ex aëre exhaūrire . . . etenim observatione compertum habeo animal una cum lucernâ in vitro inclusum haud multo plus quam dimidium temporis istius quo aliàs viveret spiraturum esse. Quod vero animal aliquandiu post lucernam extinctam vivere possit ratio haec esse videtur: lucerna non nisi continuo, eoque satis amplio et veloci particularum nitro-aëriarum flumine sustinetur: unde fit quòd nisi particularum nitro-aëriarum series vel momento temporis interrumpatur, aut eadem debitâ copiâ non suggerantur, flamma mox concidit, expiratque . . . at vero animalibus pabuli aërii penus minutior, isque per vices ingestus sufficiet; ita ut animal particulis aëriis post flammæ extinctionem residuis sustentari possit . . . atque hinc est quòd aër in quo animal suffocatur plus quam duplo magis quoad extensionem contrahitur, quam is in quo lucerna expirat."

sphere. For so difficult did Scheele find it to interpret his own experiments, that when he had in his hands the “*liver of sulphur*” which had produced a given diminution in a given volume of air,—when he had found the specific gravity of the diminished air to be less than that of common air, and the “*fire-air*,” which he had succeeded in separating from numerous substances, to have a greater specific gravity, as well as a greater power of supporting combustion,—when by reuniting them he had recomposed an air with all the properties of common air restored,—when he had arrived at the conclusion—“that the air consists of two different kinds of elastic fluids,” and that the “*fire-air*” makes between a third and a fourth of the whole bulk,—when coming finally to the ultimate question of the analysis, he failed to find the “*lost air*” in the *liver* of sulphur,—then he gave the reins to his imagination, and embracing the idea, that heat is a compound of “*fire-air*” with an imaginary substance invented by Stahl, he concluded that by the action of a “*double affinity*” the “*fire-air*” in his experiment had combined with the *phlogiston* of the *liver* of sulphur, and that the compound had passed through the pores of the glass by which it had before been confined. Where weight disappears, analysis is impossible. So he left the composition of the atmosphere to be demonstrated by those who believed, with Mayow, that elastic fluids cannot penetrate glass, and who took the pains to weigh both the air and the substances by which it was diminished; whilst he went on pursuing the phantom of his imaginative genius to the examination of imponderable essences, and the great discovery of the chemical forces of light, and of the distinctions between the heat of contact, and the heat of radiation\*.

But what shall we say to the improvements of Priestley on the principles of Mayow? Priestley—who many months after he is said by you, and others, to have discovered oxygen gas, tells us himself, that he “had no doubt it had all the properties of genuine common air.” On the 1st of August 1774, Priestley with a burning-glass, following the method of Boyle, collected this gas, and observed “that a candle burnt in it with

\* Scheele’s Experiments on Air and Fire.

a remarkably vigorous flame, but did not give sufficient attention to the circumstance at that time—that the flame of the candle, besides being larger, burnt with more splendour and heat, than in nitrous air exposed to iron or liver of sulphur.” In the October following, “I mentioned,” he says, “my surprise at the air I had got, to M. Lavoisier, but at the same time had no suspicion that it was wholesome, so far was I from knowing what it was that I had really found, and taking for granted that it was nothing more than such kind of air as I had brought nitrous air to be by the processes above-mentioned.” He mentioned it also to all his philosophical acquaintance at Paris and elsewhere, “having no idea at that time to what these remarkable facts would lead.” On the 19th of November however, having agitated it in water, he “found that a candle still burned in it as well as in common air,” though after “the same degree of agitation phlogisticated nitrous air would certainly have extinguished a candle.” “In this ignorance,” he adds, “of its real nature I continued from this time to the 1st of March following.” “But in the course of this month I not only ascertained the nature of this kind of air, though very gradually; but was led by it, as I then thought, to the complete discovery of the constitution of the air we breathe. Till this 1st of March 1775, I had so little suspicion of its being wholesome, that I had not even thought of applying to it the test of nitrous air;” “but it occurred to me at last to make the experiment, and putting one measure of nitrous air to two measures of this air, I found not only that it was diminished, but that it was diminished quite as much as common air, and that the redness of the mixture was likewise equal to that of a similar mixture of nitrous and common air. After this I had no doubt but that the air from *merc. calcinatus* was fit for respiration, and that it had all the other genuine properties of common air. But I did not take notice of what I might have observed, if I had not been so fully possessed by the notion of there being no air better than common air, that the redness was really deeper, and the diminution something greater, than common air would have admitted. *I now concluded that all the constituent parts of the air were equally and in their proper proportion imbibed in the preparation of this*

substance, and also in the process of making red lead\*,"—a conclusion identical with the ideas of Rey in 1630.

The next step in Priestley's inquiry was the employment of Mayow's mice, which convinced him that this air was *longer respirable* than common air; but his ideas of it were less accurate than Mayow's, for instead of considering it, with him, *a constituent part of nitric acid*, he thought it *a compound of nitric acid and earth*; and in December 1777, "no doubt remained on his mind, that atmospheric air, or the thing that we breathe, consists of the nitrous [nitric] acid and earth, with so much phlogiston as is necessary to its elasticity, and likewise so much more as is necessary to bring it from its state of perfect purity to the mean condition in which we find it."

You now see the error into which you have fallen when you represent Priestley as discovering *before Lavoisier* that "this was a gas wholly different from all other gases formerly known," and may perhaps suspect that you are not justified in condemning as "*an unworthy and lamentable proceeding*" on Lavoisier's part, "*the intruding himself into the history of this discovery, knowing that Priestley was the sole discoverer*†." A property of this gas, which under Priestley's observation had led to nothing, in the hands of Lavoisier gave rise to one of the most important investigations in the annals of chemistry; he, it appears from your own admission, had ascertained the relations of this elementary substance to various bases, and established that it is "the most respirable part of the atmosphere," between August 1774 and March 1775, at which date the foregoing extracts show the "*sole author of the discovery*" to have "had no doubt that it had all the genuine properties of *common air*," and that "all the constituent parts of the air were equally imbibed in the preparation of *mercurius calcinatus*, and the process of making red lead." Whoever may be called the discoverer of oxygen, whether Hook and Mayow, who first inferred its existence in nitre and in air, —or Boyle, who first *disengaged* the elastic gas from *minium*, —or Hales, who *collected* it from the same material,—or Nieu-

\* Experiments and Observations on different kinds of Air, vol. ii. p. 113, ed. 1790.

† Life of Lavoisier, p. 248.

went, who attributed its elasticity to “the expansion of the fire particles lodged in the minium, supposing fire to be a particular fluid which maintains its own essence and figure, remaining always fire, though not always burning,”—or Priestley, who observed that it supported combustion,—or Lavoisier, who distinguished it as a gas, *sui generis*, and determined its principal combinations,—if the question be, which of these names deserves the highest place in “*the history of this discovery*,” a philosopher I apprehend might be apt to hesitate,—especially perhaps between those which stand *first* in the list, and that which stands *last*.

But you have made a greater mistake in attributing to Priestley the discovery of *nitrogen*\*; and in that mistake have again wronged Cavendish of his due. Had you taken the trouble to read a paper on this subject which I have published from his MSS.†, you would have found that the same philosopher, who exceeded all his contemporaries in analysing the air with accuracy, was the first who demonstrated it to contain, after burning, a mephitic gas, incapable of supporting combustion, and *distinct from fixed air*: you would have known that, some time before Priestley’s publication in the Philosophical Transactions of March 1772, Cavendish communicated to him this paper, containing all the details of an experiment, in which a measured volume of air, confined under water, was passed backwards and forwards through a bent tube filled with powdered charcoal, and heated red-hot—the absorption was found to be definite, and the total loss of volume was ascertained—the fixed air was separated by soap-leys—the volume separated was observed and deducted—the specific gravity of the residual gas was examined, and it was found “rather lighter than common air;” lastly, it was found to extinguish flame, but to extinguish it, by the criterion of the watch, more slowly than fixed air. I know that you would be far from conceding to me that any experiment, however skilfully devised, carries with it its own conclusions: but then you would have known too the very words in which Cavendish conveyed those conclusions to Priestley—“The natural mean-

\* This discovery has been also erroneously assigned to Rutherford.

† Report of the British Association, Append. to Address, p. 63.

ing of *mephitic* air is any air which suffocates animals; and this is what Dr. Priestley seems to mean by the word: but in all probability there are *many kinds of air* which possess this property: I am sure there are *two*—namely *fixed air*, and *common air in which candles have burned, or which has passed through the fire*. Air which has passed through a charcoal fire contains a great deal of fixed air which is generated from the charcoal; but it consists *principally of common air which has suffered a change in its nature from the fire*. As I formerly made an experiment on this subject which seems to contain some new circumstances, I will here set it down\*.”

This important communication Priestley scarcely turned to

\* “I transferred some common air out of one receiver through burning charcoal into a second receiver, by means of a bent pipe, the middle of which was filled with powdered charcoal and heated red-hot, both receivers being inverted into vessels of water, and the second receiver being full of water, so that no air could get into it but what came out of the first receiver and passed through the charcoal. The quantity of air driven out of the first receiver was 180 oz. measures, that driven into the second receiver was 190 oz. measures. In order to see whether any of this was fixed air, some soap-leys were mixed with the water in the basin into which the mouth of this second receiver was immersed: it was thereby reduced to 166 oz.; so that 24 oz. measures were absorbed by the soap-leys, all of which we may conclude to be fixed air produced from the charcoal; therefore 14 oz. of common air were absorbed by the fumes of the burning charcoal, agreeable to what Dr. Hales and others have observed, that all burning bodies absorb air. The 166 oz. of air remaining were passed back again in the same manner as before, through fresh burning charcoal into the other receiver: it then measured 167 oz. and was reduced by soap-leys to 162 oz.; so that this time, only 5 oz. of fixed air were generated from the charcoal, and only 4 oz. of common air absorbed. The reason of this was, that since the air was rendered almost unfit for making bodies burn by passing once through the charcoal, not much charcoal could be consumed by it the second time; for charcoal will not burn without the assistance of fresh air, and consequently not much fixed air could be generated, nor much common air absorbed. The specific gravity of this air was found to differ very little from that of common air; of the two it seemed rather lighter. It extinguished flame, and rendered common air unfit for making bodies burn, in the same manner as fixed air, but in a less degree, as a candle which burnt about 80" in pure common air mixed with  $\frac{55}{55}$  of fixed air, burnt about 26" in common air, mixed with the same portion of this burnt air.” The gas thus obtained by Cavendish was nitrogen, with perhaps  $\frac{1}{18}$  of carbonic oxide.

better account than that which he afterwards received from the same skilful friend, of the composition of water; he quotes it indeed explicitly, but most defectively, in his paper in the Phil. Transactions of 1772. "Mr. Cavendish," he says, "favoured me with an account of some experiments of his, in which a quantity of common air was reduced from 180 to 162 oz. measures, by passing through a red-hot iron tube filled with the dust of charcoal: this diminution he ascribed to such a *destruction* of common air as Dr. Hales imagined to be the consequence of burning: Mr. Cavendish also observed that there had been a generation of fixed air in this process, but that it was absorbed by soap-leys: this experiment I also repeated, with a small variation of circumstances, and with almost the same result." He takes no notice of the distinction\* established in Cavendish's paper between "fixed air," and "common air in which candles have burnt or which has passed through the fire;" and so entirely does he misunderstand, or disregard, Cavendish's intimation of the relative levity of the latter when purified from fixed air by caustic potash, as to "conclude, after making several trials, that the air in which candles have burned" (without having been subjected to such purification) "*is rather lighter than common air*;" whilst with regard to the *lost air*, which the paper communicated to him described as "*absorbed by the fumes of the burning charcoal*," he represents Cavendish as having ascribed that loss to the "*destruction of common air*."

Though Priestley however here proves himself *not* to have been, as you imagine, the discoverer of nitrogen, this fruitful experimenter gave in the same document three original and pregnant notifications; for he announced in it—1. the effect of vegetables in restoring the respirable quality of the air; 2. the application of the known absorbing power of ni-

\* "I could not find any considerable difference in the specific gravity of the air in which candles or brimstone had burnt out. I am satisfied however that it is not heavier than common air, which must have been manifest if so great a diminution of the quantity had been owing, as Dr. Hales and others supposed, to the elasticity of the whole mass being impaired. After making several trials for this purpose I concluded that air thus diminished in bulk is rather lighter than common air."—*Phil. Trans.* 1772, p. 164.

trous gas, as a test of that respirable quality; 3. his observation that candles burn with an enlarged flame in the gas produced by the distillation of nitre\*. This observation it is, from which those who call him the discoverer of oxygen should date the discovery: for he knew as much of the gas from nitre in 1772, as of that from minium in 1774. It was the application of nitrous gas here stated, which led, in 1780, in the hands of Cavendish, to the first accurate analysis of the atmosphere, and in 1781 to the solution of the great problem—what becomes of the air lost in the combustion of hydrogen gas?

In scientific value doubtless there can be no comparison between the experimental inductions of Cavendish, or Lavoisier, and the inferences of those earlier philosophers whose speculations we are engaged in investigating: but when we follow Mayow's deductions from the assumption, on probable grounds, that there exists in the atmosphere a gas which in the act of combining with other bodies produces the phænomena of combustion,—when we observe him concluding the identity of his gas with one of the components of *nitre* from the atmospheric production of that salt, and from the sameness of its effect in enabling substances to burn,—when we further observe him determining it to be fixed in *the acid component* † of nitre, and

\* “All the kinds of factitious air on which I have yet made the experiment are highly noxious, except that which is extracted from saltpetre or alum; but *in this even a candle burned just as in common air*. In one quantity which I got from saltpetre a candle not only burned, but *the flame was increased*, and something was heard like a hissing, similar to the decrepitation of nitre in an open fire; this experiment was made when the air was fresh made, and while it contained some partieles of nitre which it would probably have deposited afterwards.”—*Phil. Trans.* 1772, p. 245.

† “Porro neque probabile est particulas istas igneo aërias nitrum quodam *perfectum* esse, uti vulgaris fert opinio; etenim suprà ostensum est non ipsum nitrum totale, sed tantum partem ejus aliquam in aëre residere; secundo arbitrii fas est, particulas aërio-igneas ad flammam quamcunque sustinendam necessarias in sale nitro hospitari, parte inque ejus magis activam igneamque constituere; quippe annotare est, nitrum sulphuris admixtum in vitro aëre vacuo, item subter aquas, satis promptè deflagrare.” “Quapropter cum nitri pars aliqua ab aëre oriatur, et particulae aëris igneæ in eodem existant, statuendum esse videtur partem nitri aëriam nihil aliud quam particulas ejus nitro-aërias esse. Jam vero cum pars nitri

supporting this view of the subject by alleging the sameness of the effect of nitric acid and of the burning-glass, in adding to antimony weight and specific medical properties,—when we find him extending these views to other substances, stating with most remarkable accuracy the acidification, in various cases and degrees, of sulphureous and fermenting substances by atmospheric exposure, and hence inferring that this gas is the principle not only of combustion but of acidity\*,—when

aërea in spiritu ejus acido existat, non vero in sale fixo, quod reliquam nitri partem constituit, uti supra ostendimus, concludere licet particulas igneo-aëreas nitri, quæ cum parte ejus aëreâ idem sunt, in *spiritu nitri* reconditas esse.”—*De parte aëreâ igneâque Spiritus Nitri*, pp. 12. 17.

\* After stating (*Ibid.* p. 37) that the acid of oil of vitriol is not due to any acid already existing in sulphur, of which he says there are no signs, he adds—“ potius putandum est, particulas ignis nitro-aëreas, in longâ illâ distillatione vitrioli, cum sulphure metallico Coleotharis congregari et effervescent; unde fit quod particulæ sulphuris istius salinæ, inter particulas ignes mutuo se atterentes interpositæ, contundantur et comminuantur, ita ut eædem tandem exacuantur, et ad fluoris statum perducantur, quæ demum ignis vi in altum delatæ, oleum vitrioli componunt, haud multum secus ac spiritum sulphuris (sulphurous acid gas) per deflagrationem ejus fieri supra ostendimus.” “ Si vitriolum ad totalem spiritus acidi expulsionem calcinatum, aëri humido aliquandiu expositum fuerit, idem spiritu acido de novo imprægnabitur. Nempe spiritus nitro-aëreus cum sulphure metallico *Coleotharis* lentè congregatur, motuque obscuro cum eodem effervescit; unde fit, quod particulæ salinæ, aut metallicæ, sulphuris istius, modo supra dicto, ad fluorem perducantur. Profecto vix concipi queat quâ aliâ ratione spiritus iste vitriolicus in Coleothare produceretur; neque enim idem in Coleothare mox e distillatione extitit; neque putandum est eum *totaliter* ab aëre prosapiam ducere, ut alibi ostensum est.” “ Vitriola e lapide seu potius glebâ salino-sulphureâ (vulgo Marchasitum vocant) conficiuntur, e quâ igni commissâ flores sulphuris vulgaris, copiâ satis amplâ, eliciuntur: postquam autem gleba ea aëri, astrisque pluviis, aliquandiu exposita est, et dein, prout ejus fert natura, sponte suâ fermentata est, eadem vitriolo ubertim imprægnabitur: nimis rurum spiritus nitro-aëreus cum sulphure metallico *marchasitarum* istarum effervescent, partem earum fixiorem in liquorum acidum convertit, qui mox ab ortu suo particulas metallicas lapidis adoritur, evocatque; tandemque cum iisdem in vitriolum coalescit. Quinetiam *Rubigo ferri*, quæ naturalem vitriolicam obtinet, particularum nitro-aërearum cum sulphure ferri metallico congregentium actione produci videtur.” “ Anmadvertendum est insuper, quod non tantum in rebus solidis, sed etiam in liquoribus, sal acidum, sive *achor*, spiritus nitro-aërei actione producatur.” “ Præterea nescio an non spiritus acidi e lignis ponderosis distillati, simili ratione per ignis operationem inter distillandum fiant.” “ Illud etiam obiter

by an induction of the like kind we find him showing it to us as the principle by which metals gain weight from the air, and vegetables germinate and grow, and undergo an obscure fermentation [*æstum obscurum*] in their life and their decay\*, —lastly, when we find him ascribing to the same principle the phænomena of respiration, and representing the reduction of this gas from the elastic to the fixed state by its union with the blood, in the lungs and elsewhere, as the cause of its change of colour, its heat and its aptness for stimulating the heart and exciting muscular motion†—in contemplating so

annotamus, quod spiritus acidi e saccharo et melle distillati haud multum absimili ratione, per actionem spiritus nitro-aërei ignei, fieri videantur.” “ Liquorum autem fermentatio in eo consistit, quod particulæ nitro-aëreæ aut *liquori insitæ*, aut *aliunde adrenientes*, cum particulis liquoris salino-sulphureis [basic] effervescunt.” “ Huc etiam spectat, quod vina, aut cerevisia generosiora, radiis solaribus diu exposita, aut in loco calido detenta, processu temporis in acetum commigrant.” “ Ex iis quæ dicta sunt haud difficile erit intellectu quomodo spiritus acidus nitri in terrâ generatur.” “ Et ita demum ostendere conatus sum, quod salia quæcunque acida a particulis salinis, spiritus nitro-aërei ope, ad fluorem sive fusionem evectis producantur.” “ Quoad differentiam liquorum acidorum, eam a diversitate salium e quibus iidem constituantur procedere putandum est, uti etiam ex eo, quod salia fixa, nunc magis, nunc vero minus a spiritu nitro-aëreo alterantur, exacuunturque; et tamen inter salia acida quæcunque affinitas magna est et similitudo; inque iis omnibus particulæ nitro-aëreæ igneæque veluti in subjecto idoneo hospitantur.” “ Particulæ terræ salinæ hoc modo ad fluorem evectæ hospitium idoneum fiunt, in quo particulæ nitro-aëreæ recondantur detineanturque: ab iis autem utrisque strictim unitis spiritum nitri, qualis distillatione elicetur constitutum esse arbitror.”

\* “ In ortu vegetabilium, spiritus nitro-aëreus, in motu et vigore positus, sulphur in statu fixo existens adoritur, quo tandem ad volatilitatem perdueto, spiritus nitro-aëreus in salinis vinculis incarceratus figitur.” “ Nostra fert opinio etiam fermentationem ad vegetabilium interitum tendentem a particulis nitro-aëreis et salino-sulphureis, se invicem commoventibus, procedere.” “ Spiritus nitro-aëreus a conjugi suâ salinâ violenter abruptus motu suo impetuoso omnia perturbat, mixtique compagem solvit.” “ Ea quæ spiritum nitro-aëreum excludunt res a corruptione vindicant.” He instances fruits, flesh and butter, as being preserved from putrefaction, and iron from rust, by things which exclude this gas, especially inflammable things, such as oil.

† “ Quemadmodum particulæ nitro-aëreæ terræ spiracula lente subeuntes, ibidem cum particulis salino-sulphureis, iis vero immaturis, æstu obscuro congredivintur, a quo vegetabilium vita dependet—ita particulæ eadem nitro-aëreæ magis confertim in crux massam pulmonum ministerio

just and splendid a generalisation, running parallel to the whole range of chemical induction on all those subjects which occupied the succeeding century, it is impossible not to allow that this young man handed down a bright light to all who followed him\*, and made more of a few facts, than the greater part of the next generation did of many.

Mayow also examined the two kinds of air which Boyle had obtained by the action of the nitric and vitriolic acids on iron, and observed the permanence of the one gas and the partial condensation of the other. To determine whether they resembled common air in containing any of the nitro-aërial *aura*, he added them to air in which a mouse was confined, and inferred that they do not, from their not prolonging the animal's life. He then examined their relative elasticity, and finding in them the same capacities of compression and expansion as in common air, he decided that there exist various elastic fluids, and held with Newton that these, as well as that particular *aura* which he deemed pre-eminently elastic, and the residual gas from which it is abstracted by respiration, owe their different degrees of elasticity and permanence to elementary differences in their particles, and in the substances from which they are derived†.

introductæ, particulisque ejus salino-sulphureis ad justum vigorem evectis quoad minima admixtæ, fermentationem satis insignem, qualis ad vitam animalem requisita est, efficiunt."—p. 147. He states that the colour of arterial blood has been shown by Lower to be owing to the admixture of air with it in the lungs, and lays it down, that the heat of the body is due to the combination of these nitro-aërial particles with the blood, and the increased heat in exercise to a greater number being breathed in the same time. In like manner he accounts for febrile heat, for acid in the blood and urine, for the digestion of the food, and for muscular contraction.

\* Mayow's work, besides its publication in England, was at least twice reprinted abroad; a detailed account of it was given in the Philosophical Transactions. It was repeatedly quoted by Hales, whose book was in every chemist's hands, and by other authors: it was therefore sufficiently known to have produced a real influence on the minds of men.

† *De Spir. Nit.* cap. 9. p. 163.—"Utrum aër de novo generari possit?"—In his account of Boyle's gases he says—"aura prædicta haud minori vi elasticâ quam aër vulgaris donatur prout sequenti experimento mihi compertum est." "Simili ratione experimentum feci, num aër in quo animal, aut lucerna expirassent, æquè ac aër inviolatus, vi elasticâ pollent; et

The only philosopher, as far as I am aware, who dissented from these views, was the elder Bernoulli : having detailed his own views respecting fixed air\*, “ Mayow,” he said, “ after

quidem mihi videtur aér iste haud minus quam aér quivis alius se expandere.” “ Quanquam *aura* ista in quâ animal aut lucerna expirarunt vi elasticâ tæque ac aér inviolatus pollet, et tamen eadem particulis nitro-aëreis vitalibusque destituitur.” “ Hic etiam referre possumus quod in cap. sup. de *auræ* hujusmodi aërisque vulgaris differentiâ annotavimus, et tamen verisimile est *auræ* istiusmodi cum aëre vulgari magnam affinitatem intercedere, vimque elasticam eorum ntrorumque a causâ haud multum diversâ provenire. Etenim cum ferrum e particulis rigidis, item spiritus corrosivi e particulis nitro-aëreis summè elasticis constant, *aura* ex iis utrisque invicem fervescientibus conflata ab aëre vulgari haud multum diversa erit.”

\* “ Allata experimenta satis, ni fallor, ostendunt existentiam aëris in corporibus, sed et alterum nobis ostendendum est, nimirum quod aér iste sit aëre naturalis consistentiæ densior. Hoc autem sequenti experimento demonstratur. Simatur vasculum liquore quodam acido semiplenum, ut A. C. D. B., et tubus aliquis vitreus E. F., alterâ parte E. clausus, alterâ vero F. apertus, impleatur eodem liquore; hujus vero orificio F. induatur globulus G., de luto, vel cretâ, in quibus nempe multæ particulæ alkali insunt, confectus; statimque, indice super orificium F. posito, invertatur tubus; et liquori in vasculo contento immagratur orificium F.: amoto digito, mox observabitur magnam effervescentiam excitari, quæ per aliquot horas durabit, donec omnis aér, intra particulas alkali contentus, solutis vinculis quibus coarctabatur, ad superiora ascenderit, et materia subsederit; tum demum animadvertisit, aërem hunc, postquam despumaverit, in supremâ parte depresso liquore, magnum spatium E. H. occupare: quandoquidem autem superficies H. liquoris in tubo altior est superficie liquoris in vasculo, erit aér in spatio E. H. contentus aliquantulum rarer aëre externo; proinde, ut fiat ejusdem consistentiæ, opus est ut aut tubus altius immagratur, aut plus liquoris assundatur, donec superficies interna coincidat cum superficie exteriori; quo facto, erit quidem spatium E. H. priori paœlulum contractius, et aér in eo contentus naturalis consistentiæ: nihil minus tamen adhuc magis erit, duplo, triplo, quadruplo, (pro diversitate materiæ terrestris ex quâ globulus conficitur, quâ scilicet plus vel minus particularum alkali in se continet,) quam quod tota moles globuli G. occupat; quod certum indicium est aërem istum, cum omnis adhuc in globo continebatur, multo densioreni fuisse quam aér externus est: posito enim globulum constare, ex unâ parte, materiæ terrestris, et ex unâ parte, pororum, quibus nempe aér condensatus inest,—vel, quod eodem recidit, spatium quod materia terrestris occupat esse æquale spatio quod aér in poris contentus replet,—si nunc spatium E. H. sit duplum spatii globuli totius, sequitur, aërem in globulo contentum quadruplo densiorem esse quam est aér externus, si triplo sextuplo, si quadruplo octuplo, et sic porro in subduplâ ratione; si vero ponatur spatium materiæ terrestris non esse æquale spatio pororum, sed in aliâ ratione

various experiments concluded, that the substance itself of the globule, from which air was produced in them, is changed into an ‘aura,’ as water is changed into vapour by heat: but unlike vapour, this *aura* remains *aura*, and as he himself proves by experiment, retains its elastic force. Does there exist then any other body besides air which is fluid and endowed with elastic force? I scarcely believe it. He alleges indeed as a reason for denying to this *aura* the nature of common air, that he has found by experiment that the said *aura* is incapable of supporting life: as if, because it does not support life, therefore it cannot be air! we see our atmospheric air itself in time of pestilence unfitted to support life: has it therefore ceased to be air? it would be absurd to say this. It is not to be denied that in the space E. H. [of a glass tube filled with carbonic gas] other particles besides air find room, separated perhaps by the impetuous motion of the effervescence from the acid liquor, or the solid globule, and carried up with the air. Nor can we wonder that such an air, filled with miasma, if breathed by animals, cannot keep them alive, especially when it is obvious that the spirit of nitre, and the globule of iron, used by the distinguished author, abound in many impure and poisonous particles, which if introduced into the system in breathing, may well corrupt the mass of the blood and induce death. If instead of the spirit of nitre he had chanced to use another acid liquor of a more *benign* quality, for instance the spirit of vitriol, and instead of a globule of iron, had taken one of an earthy kind, as in my experiment, the animal doubtless would not have perished, or at least would have lived longer. So that we may collect from this, not that the air, *as air*, destroyed the animal, but only incidentally, so far as it abounds with particles of a different kind and unfit for the support of life. But that we may make certain of one fact—namely that the substance of the globule itself is not changed by the fermentation into air, but that air really pre-existed in the globule, and was therefore *not* generated anew, the following experiment may be tried. Let the

majoris vel minoris inæqualitatis, densitates aëris in globulo æque facile ad calculum revocari possunt.”—Bernoulli, *Dissertatio de Effervescentia et Fermentatione*, No. 1. p. 20. 1690.

weight of an earthy globule, well-dried, be taken with perfect accuracy before the effervescence: then after the effervescence, when all the particles of the globule subside to the bottom, let the whole mass of the globule, which now lies dispersed, be carefully re-collected from the liquor; and let it be well-dried as before: lastly, let the weight also of the dried material be accurately ascertained by the help of the balance: this done, we shall find that the substance of the globule has lost nothing of its weight, or at least scarce a hundredth part, which perhaps exhaled with the air during the effervescence. But according to Mayow, it ought to have lost by far the greatest part of its weight; since it follows from his hypothesis, that the whole body of air occupying the upper part of the tube was taken from the substance of the globule; and so its weight should have been notably diminished, which nevertheless is contrary to experiment."

In this criticism Bernoulli overlooked the chief fact on which the theory of Mayow rested—the constant diminution of the volume of common air, when breathed or burnt. And his attempt at an experimental refutation of it may serve to convince you of the danger which the greatest men may incur when they venture on deciding chemical questions without a knowledge of chemistry. To give the utmost credit to the alleged result of his experiment we must presume the "acid liquor" employed in it to have been *oil of vitriol*: but any boy in a chemist's laboratory could have told him that the vitriolated lime which he collected at the end of the experiment was a different substance from the chalk with which he began it, and that the consequence therefore which he drew from the weight remaining the same was a fallacy. The compounds of sulphur, I perceive, are a stumbling-block even to you\*: to Bernoulli's reputation as an experimental philosopher they are more fatal than he deemed them to animal life; for the wholesomeness of the air from so "benign" an acid as oil of vitriol, and "a globule of the earthy kind, as in his own experiment," was an assumption which the first trial would have discovered to be false. But it is more surprising that the computations, congenial to his own studies, which he pro-

\* Note to the Lives of Cavendish, Watt and Black, vol. ii. p. 511.

ceeded to make, of the amount of condensation of the air in the pores of the chalk, should not have apprised him that the globule on which he experimented *must have lost weight*, when so great a volume of condensed air was disengaged from it.

At a later period the younger Bernoulli paid so much respect to his father's opinion as to speak of the multiplicity of airs as a doubtful question. "The question," he says, "has long been agitated, whether the factitious elastic *aura* brought out of bodies, is ordinary air, or not; which question I shall not decide. If however the air of gunpowder be taken to be 1000 times denser than natural air, and 10,000 more elastic, then it follows from what precedes, that air compressed by an infinite force cannot be condensed more than 1331 times, and according to the same rule the elasticity of an air four times more dense than the natural air would be to the elasticity of natural air as  $4 + \frac{1}{4}$  to 1. But whether the experiments instituted by others, which make the ratio of these elasticities as 4 to 1, were conducted with sufficient accuracy, and whether the heat of the air, whilst under pressure, remained the same, I know not. It is probable however that the same *aura* which lies latent in the pores of the gunpowder is the cause of the elasticity of elastic bodies, and contractile *villous* materials; for when bodies are reduced by any force to an unnatural form, the elastic *aura* abounding in the little vacuities is compressed, and in giving the form of greatest space to those vacuities brings the body back to its original shape and extent."

In the English school, however, of pneumatic chemistry, and in the chief successor to the views and experiments of Boyle and Hook, of Mayow and Newton, there was no hesitation on this point. You have quoted the opinion of Hales, as representing, *instar omnium*, the general notion among experimental philosophers before the time of Black, that air was a single and simple element; and your inaccuracy on this point is not to be wondered at; because Hales's opinion has been over and over again mis-stated, even by eminent chemical writers. Those however, who are better occupied in making scientific discoveries than in reviewing them, may be excused, if they appear to be often less exactly acquainted with the opinions of others than with their own, so far at least

as we can fully acquit them of desiring to exalt their own views, or the views of a particular æra, or a favourite author, by underrating all that has gone before.

The mistake in this case has certainly in great measure arisen from the circumstance, that the inquiries of Hales were directed more to the generic and physical properties of gases, than to their specific and chemical distinctions. He calls “airs generated in effervescences”—“true permanent air”: he has been supposed to mean that they are true *atmospheric* air: his real meaning was—that they are true elastic fluids, and, with the same permanence of constitution, possess the *same elastic force* as common air. This important fact had been before announced by Mayow, but was first ascertained with precision by Hales. “That I might,” he says, “with the greater degree of certainty be assured of the degrees of compressibility of these different airs, I divided the capacities of two equal tubes into quarters of cubic inches, by pouring severally those quantities of water into the tubes, and then cutting notches with a file on the sides of the tubes at the several surfaces of the water; by which means I could see, by the ascent of the compressed water in the tubes, that both the factitious and common air were exactly alike compressible in all degrees of compressure, from the beginning till they were loaded with a weight equal to that of three atmospheres, which was the furthest I durst venture for fear of bursting the glass\*.” Having made this contribution to our knowledge of the physical properties of the gases, and established that at common pressures and temperatures “with equal weights they are compressed exactly in the same proportion with common air,” he went on to examine whether there exists any difference of specific gravity between the air and them; but contenting himself with the single experiment to which I have already referred, where no difference could be detected†, he left to Cavendish the grand discovery of the distinctions of density in elastic fluids; and it may possibly increase your respect for that discovery to remark, that his false conclusion led him into much error in computing the weight of aërial substance fixed in va-

\* Stat. Essays, Append. p. 314.

† Analysis of the Air, Exp. 77.

rious bodies from the volume which they yielded, on the supposition that the density of all airs is the same.

Hales however rendered essential service to what may be more strictly called the *chemical* philosophy of aërial fluids. I have before noticed that we owe to him the discovery of a fact in gaseous chemistry, the consequence of which it is impossible to overrate—the condensation of atmospheric air by nitrous gas, in such a manner that *the two gases were observed by him to occupy the same space*. He first also determined with *numerical* exactness, and by very ingenious methods, the volume of air absorbed in a variety of chemical processes, and stated in the clearest terms the chemical nature of that *absorption*,—a statement adopted, as I have shown, by Cavendish, and strangely misconstrued by Priestley. “They were changed,” he says, “from a repelling elastic to a fixed state by the strong attraction of other particles which I call *absorbing*.” He taught the chemists of the succeeding generation how to procure almost all the gases which formed the subjects of their investigation; and he taught them also the more important lesson of conducting those investigations by *measure* and *weight*. Some of his experiments led directly to the most important conclusions at which they arrived. It was not for nothing that he observed that the “*Sal Tartar*” (very highly calcined) with which he essayed to purify the air for respiration had “absorbed one-third of the fuliginous vapours which arose from the burning candle\*,” or that he recorded experiments on phosphorus, in which “2 grains, fired in a large receiver, flamed and filled the retort with white fumes, expanded into a space equal to 60 inches, and absorbed 28 cubic inches of air;” and “when 3 grains were weighed soon after it was burnt, it had lost half a grain of its weight†.”

It is true that he made no advance towards analysing the air: and further, he argued that “the sudden and fatal effect of noxious vapours, which has hitherto been supposed to be *wholly* owing to the loss and waste of the *vivifying spirit of air*, may not unreasonably be *also attributed*” to other causes. “If,” he says, “the continuance of the burning of a candle be *wholly* owing

\* *Analysis of the Air*, edit. 1727, p. 272.

† *Ibid.* p. 169.

to the vivifying spirit, then supposing, in the case of a receiver capacious enough for a candle to burn a minute in it, that half the vivifying spirit be drawn out with half the air in 10 seconds of time, the candle should not go out at the end of those 10 seconds, but burn 10 seconds more; which it does not, therefore the burning of the candle is not wholly owing to the vivifying spirit, but to certain degrees of the air's elasticity,"—a principle which he goes on to illustrate by the "common observation, that in very cold frosty weather fires burn most briskly\*."

But we are by no means to conclude from hence that Hales had any doubt of the plurality of elastic fluids; on that point he quotes, as at once the foundation and the result of all his inquiries, the opinions expressed by Newton in the Optics:—"The illustrious Sir I. Newton," he begins, "observes, that true permanent air arises by fermentation, or heat, from those bodies which chemists call fixed, whose particles adhere by a strong attraction, and are therefore not separated or rarefied without fermentation, those particles receding from one another with the greatest repulsive force, and being most difficultly brought together which upon contact are most strongly united." "Dense bodies by fermentation rarefy into *several sorts of air*, and this air by fermentation, and sometimes without it, returns into dense bodies†," "of the truth of which," Hales adds, "we have proof from many of the following experiments." And as he begins, so he ends: for having again repeated the same quotation from Newton, he closes his "*analysis of air*," by drawing this general inference from all his researches—"Since we find in fact from these experiments that air arises from a great variety of dense bodies both by fire and fermentation, it is probable they may have very different degrees of elasticity in proportion to the different size and density of their particles, and the different forces with which they were thrown off into an elastic state."

What now, give me leave to ask, becomes of your statement, that "when D'Alembert wrote the article '*Air*' in the Encyclopédie in 1751, he gave the doctrine then universally received, that all the other kinds of air were only impure

\* *Analysis of the Air*, edit. 1727, p. 247.

† *Ibid.* p. 312.

atmospheric air, and that this fluid alone was permanently elastic?" You tell us elsewhere that D'Alembert disregarded inductive philosophy, and professed himself ignorant of chemistry: and thus I should have accounted for his ignorance on this point, if I had not found on consulting the volume which you quote, that he really expressed *no such opinion* respecting air, and moreover has stated fully the views entertained of it by those, who in his own words, "supposent qu'il peut être produit et engendré, et que ce n'est autre chose que la matière des autres corps, devenue par les changemens qui s'y sont faits, susceptibles d'une élasticité permanente." D'Alembert says indeed, that *some of the ancients* considered the air as a simple element, but remarks with truth, that they did not attach the same sense to that term as ourselves.

I have now completed the sketch which I promised, of the gradual advance of this branch of science, in the æra beginning with Boyle and ending with Hales, during which the prevalent theory *multiplied the number of gases beyond the truth*, by supposing them as numerous as the substances from which they were obtained. This may be regarded as the æra of the first regular school of inductive science (if we except the less perfectly methodised school of Galileo), instituted by the original members of the Royal Society, for the professed purpose of executing the grand design of Bacon.

We have been lately told by a very able and lively writer, that the sole use and effect of the *Novum Organum* of the great founder of this school, was to bring down philosophy from high but barren aims to the level of common utility,— that "the inductive method has been practised ever since the beginning of the world,"—that "it is not only not true that Bacon invented it, but that it is not true that he was the first who correctly analysed that method and explained its uses,"—and that "he greatly overrated its utility;" we have been further told that the difference between the "*instances*" which make an absurd induction, and those which constitute a sound one, "is not in the *kind* of instances, but in the *number* of instances; that is to say, the difference is not in that part of the process for which Bacon has given precise rules, but in a circumstance for which no precise rule can be given:" and this

notion of philosophical induction is illustrated by asking, "Will ten instances do, or fifty, or a hundred? In how many months would the first human beings who settled on the shores of the ocean have been justified in believing that the moon had an influence on the tides? After how many experiments would Jenner have been justified in believing that he had discovered a safeguard against the small-pox? These are questions," it is added, "to which it would be most desirable to have a precise answer; but unhappily they are questions to which no precise answer can be returned\*." Certainly, if the force of induction, and the inquisition and demonstration of truth, *does* depend on the "number of instances," and not on "the kind," Bacon has written in vain; but *you* know, my Lord, how it was, that in the hands of one who had better studied "the inductive method," a vague and local idea, darkened with errors that destroyed its credibility and use, passed through the *mint of a very few decisive experiments* into the *treasury* of accepted truths: that which this author esteems the inductive process had been repeated thousands of times without fruit; but when Jenner, after vaccinating a child, inoculated it, and found that it resisted the virus of inoculation, the probability that "he had discovered a safeguard" rose at once, by the force even of a single experiment, to an amount which medical experience could "precisely" assign.

Mr. Macaulay considers the credulity of those whom he calls "the dupes of Mesmerism," as due, not so much to neglect of these laws of evidence, as to want of natural sagacity: but the history of science by no means justifies this view of unfounded opinions: the truth is, that all sciences, except the mathematical, had stood for centuries in the same position in which such studies as go by the names of Animal Magnetism and Craniology, appear to stand now,—the position, that is, of collections of alleged facts unscrutinized and unsifted,—of generalisations precariously deduced, and truths, where they contained any, mystified and confused.

This was the state of science when Bacon appeared. The master science of *evidence*, like every other science, requires

\* Life of Lord Bacon, p. 411.

for its perfection both *rules* and *examples*. Bacon gave the *rules*. It has been observed by one well-qualified to offer an opinion, that “he traced not merely the *outline* but the *ramifications* of science that did not yet exist\*.” But the chief, the all-pervading, ever-during benefit,—the force of direction, which he gave to the progress of knowledge,—lay in this—that he *did* “first analyse the inductive method correctly, that he first taught the specific value of every part of its evidence, and *that* with such precision,” and such impressiveness, that a great school was founded upon his writings, who have handed down from him the torch of science, and have proceeded during the last two hundred years to practise, and mature, his rules.

Yet we shall do no more than describe a real change in the history of inductive science, if we shall proceed to speak of the experimental school of the æra which commences with Black and Cavendish, as the school of Newton: for the severe reason of the mathematician, grafted on that inductive principle of simplifying, and hedging in, ideas more complex than space and number, till they are divided and narrowed to the point of demonstration, shone forth in Newton’s immortal works, and especially in his Optics, with a light as much more powerful than even the luminous lessons of Bacon, as example is more powerful than precept.

In this I believe you will agree with me, that if in our seats of learning the attentive study of *such examples of reasoning* had been made one of the essential requisites of an accomplished and sound education, we should not have seen so many educated persons, ignorant of the laws of evidence and unconscious of their own deficiency, become as easy victims as the most uninstructed, to wild paradox and blind credulity.

What the Optics were for experimental philosophy in general, that little unpretending Essay, of scarce seventy pages, which Black published in 1755, on the properties of Magnesia, was to chemistry. It was, as you say, the second instance of a most beautiful example of inductive research; and the method of reasoning pursued in it deserves to be more particularly described, as constituting indeed the highest of

\* Playfair’s Dissertation, Encycl. Brit., p. 55.

all its merits. Not one word is there here of the *sulphureous principle* of the old chemists, or the corresponding *phlogistic* of the new: but there is, observe, *one general established principle*, reigning in the experimenter's thoughts, governing his hand, interpreting every phænomenon as it presents itself, dictating every successive experiment, and bringing forth each consequent discovery in that brief and transparent investigation.

The principle by which it was thus illuminated, was *the principle of elective affinities*,—a principle, first stated as we have seen, and generalised by Newton, experimentally noticed by Mayow, with others of the early chemists, and then recently systematised and *tabulated* by the French chemist Geoffroi. And here if we adopt such expressions as yours, in calling this “an example of *strict inductive investigation*,” let us understand clearly what we mean; let us not forget that the process of what is called the *inductive* method, in its most usual applications to such a science as chemistry, does not differ from that which is called *deductive* in *mechanics*, otherwise than in the imperfect degree of our reliance on the *generality* of the laws to which it is applied: in mechanics we now assume the laws which we have observed, to be applicable to *all matter* whatever; in chemistry, when the *nature of the subject is widely different* from those on which we have experimented, we dare not trust the *certainty* of our generalisations: the firmest believer in ætherial matter would hesitate to presume on Newton's hypothesis of its possessing *chemical affinities*, as a certain truth; and *gas* was to Black what *æther* is to us. His reasonings respecting fixed air were in fact all *deductions* from the presumed principle of elective attractions; but as far as regards the chief point of his discovery—the silent transference which he remarked of the gaseous substance that, as Hales had taught him, was fixed in salt of tartar, to calcined magnesia, and again from magnesia alba to caustic lime,—*the principle* which suggested the remark and the experiments, was itself *confirmed and established, in its extension to gaseous substances*, by the *result of those experiments*. In every such course of research, whatever offers itself *fortuitously* is observed by an eye which is on the watch for the appearance

of the laws, known or assumed, that fill its meditations; and the whole *design* with which each experiment is instituted, is to test the applicability of those laws, and to try the validity, or the accuracy, of principles which have more or less the character of *foregone conclusions*.

This is experimental philosophy: this is the science of observing, interrogating, and interpreting, nature—apart from that faculty of catching *far analogies* on the wings of a lively and just imagination, which constitutes perhaps the highest part of the *genius* of a philosopher: though we should be much in error, if we regarded even this high gift of Heaven as incapable of being improved by rule, example, and use.

Thus it was that Black, under the guidance of the light which a clear conception of the laws of affinity shed over his mind, proved by a short series of experiments so devised as to eliminate, one by one, *all alternative suppositions*, the following points:—1. That magnesia is a distinct substance, having its own laws of combination to distinguish it from other earths—2. that that substance, which is sometimes found in air, and sometimes fixed both in this and other absorbent earths and alkalies, is subject to the laws of chemical composition, decomposition, and transfer—3. that common air does not enter into the same combinations as fixed air;—and lastly, he inferred from the general analogy of the effects of chemical attraction, that *unsaturated affinity* is the *form*, as Bacon would have termed it, of *causticity*. This brief, simple, and choice specimen of synthetico-analytical research, to that time unexampled in chemistry, he completed and crowned, by denoting the law of double decomposition as dependent on “*the sum of the forces*,” and fixing the place, not of magnesia only, which was as much as he at first contemplated, but of fixed air, side by side with the acids, in its own place in the *table of relative affinities*\*.

\* Essays and Obs. Phys. and Lit., vol. ii. pp. 221–24. The following description, by the French chemist De Lasône, in 1753, of the manner in which an aërial spirit is combined with lime and iron in the waters of Vichy, is worthy of notice, as a curious anticipation of truth since more exactly developed:—

“Toutes ces expériences prouvent évidemment que ces eaux sont alcalines, par un principe salin et par une terre absorbante; qu'elles contien-

Black had certainly very little ambition, and apparently little of the activity of an ardent curiosity: for here he rested, after drawing from the facts before him some pregnant inferences, as to the production, for instance, of this fixed air from charcoal, and its diffusion through the atmosphere\*. He did not even measure, or collect, the air extricated in his experiments; still less did he try its density; he did not extend his inquiries at all into its elastic condition: and the consequence was that on *that point* which you take for the *stress* of his discovery, he rather retrograded from the inferences of his predecessors than advanced beyond them: for he went no further in his conclusions than this—"Quick-lime therefore does not attract air when in its most ordinary form, but is capable of

tenant une matière ferrugineuse; qu'elles contiennent *un principe spiritueux*, composé non seulement *d'un air sur-abondant*, comme il s'en trouve dans quelques eaux, mais encore d'une portion de cette terre subtile dont nous venons de parler, jointe au principe huileux du bitume, et volatilisée par cet air, qui vraisemblablement est le principal agent qui tient cette terre suspendue, puisque lorsqu'on l'en chasse brusquement en secouant l'eau minérale, la terre se dépose aussi promptement, et qu'au contraire elle ne se dépose que très-lentement lorsque l'eau est bouché et que l'air ne s'évapore que lentement; que ce même principe contient aussi une portion de la terre ferrugineuse qui existe dans ces eaux, puisque lorsqu'elles sont dépouillées de leur air et qu'elles ont formé leur dépôt, on ne remarque plus aucun indice de matière ferrugineuse; qu'on doit encore à ce même air mêlé avec la terre et le bitume, et qu'on peut en cet état regarder, suivant la pensée de Lister, comme une espèce d'esprit, la saveur acidule qu'ont ces eaux à leur source et qu'elles perdent avec leur air sur-abondant; enfin que ce même principe aérien est la cause d'une partie de l'effervescence qui ces eaux font avec tous les acides."—*Hist. de l'Académie*, 1753, p. 174.

\* Black also observed that the peculiar matter of fixed air combines with alkalies to different degrees of saturation. Cavendish, pursuing this hint with his usual skill and accuracy in his experiments on the quantity of fixed air contained in alkaline substances (1766), determined the *weight* of carbonic gas which combines with a given weight of lime, and the weight of it which combines with the equivalent weight of potash capable of saturating an equal quantity of acid: he ascertained that potash and ammonia combine with 2 proportions of carbonic acid, of which one is about double the other, and extended the observation to magnesia: in his experiments on the Rathbone Place water (1767) he extended it also to lime, and in the 7th experiment gave the exact duplicate proportion for the composition of what is now called the bicarbonate of lime. The determination of these combining weights, and their duplicity, may be considered as having laid the foundation of the theory of *multiple proportions*.

being joined to one particular species only, which is dispersed through the atmosphere, *either in the shape of an exceedingly subtle powder*, or more probably in that of an elastic fluid\*." He did more, it is true, than discover the chemical affinity of one substance only which floats in the air, or is fixed in many earths and alkalies; for that discovery, as it limited the *number* of such substances, so it extended to the rest the probability of a *like chemical constitution*: but whether these substances are or are not *elastic*, Black, like Daniel Bernoulli, declined to decide. The demonstration of this fact—that there exists *more than one species of elastic fluid* permanent at a common temperature and pressure when not acted upon by a condensing attraction—*was reserved for Cavendish*; being the consequence of that determination of its specific gravity of which you speak so slightly.—And here again, you see that in your haste you have denied this great philosopher his due.

And now that we have not only walked together over a part of the demesne of experimental philosophy with more deliberation than your leisure seems usually to allow you, but even ventured on searching some of the inner chambers of the art of experiment, I must appeal to you, not in the style of arch solemnity with which your "illustrious colleague" addressed you in the chamber of the Institute, as having weighed the evidence in the case of Watt *versus* Cavendish—"Avec le scrupule en quelque sorte judiciaire qu'on pouvoit attendre de l'ancien *Lord Chancellor* de la Grande Bretagne†,"—but I ap-

\* Essays, Phys. and Lit., vol. ii. p. 198, 1765. Experiments on Magnesia, &c., 1777. [A reprint of the Essay, in 12mo, without alteration.]

† *Annuaire*, 1839, Note, p. 361. Lord Brougham, out of *court*, deals I fear as hastily with literature as with science; and *there* also sometimes *makes* the facts on which he reasons. Thus he criticizes as "unintelligible" the condensed sense of that well-known line, in which Johnson, in his imitation of the Tenth Satire of Juvenal, speaks of "patience" as "sovereign o'er transmuted ill;" but it is Lord Brougham who *makes* it unintelligible, by substituting from his own poetical mint—"nature," where Johnson had written "patience." (Life of Johnson, p. 76.) Again, he animadverts severely on Johnson for "roaring out, 'No, Sir !' in the presence of Hume, on being asked by a common friend to let him present the Historian to the Moralist" (Life of Hume, p. 223); and he adds, "above all we have a right to complain that the associate of Savage, the companion of his debauches, should

peal to you, as ever you have learnt the laws of evidence from the only Chancellor of England who is of authority in philosophical questions, as ever you have listened to, and comprehended, that pupil of Bacon and Newton, the beauty of whose lectures you have so vividly described,—to take some shame to yourself, for having perused, by your own confession, the notes of Cavendish, without perceiving that all which I have said of the experiments of Black, as being so connected as clearly to manifest the whole train of the experimenter's thoughts, is still more clearly true of *these*.

You know what the problem was, on the investigation of which Cavendish was intent when he made the discovery in question. You know his aim to have been to find out what was become of "*the air lost*" in the combustion of hydrogen with common air. And what were the preliminary trials by which he searched for the lost gases? He tried—1. whether they were "*changed*" into carbonic acid;—2. whether they were "*changed*" into nitric acid;—3. whether they were "*changed*" into sulphuric acid: he *negatived* by conclusive experiments these three suppositions of *condensation*: at this crisis of his inquiry Warltire burnt inflammable gas and common air in a vessel which he imagined to be close, and finding a very sensible loss of weight, concluded, with Scheele, that ponderable matter had passed through the vessel's pores in the form of heat: at the same time he repeated an observation, made also by others, that in the combustion *water* was deposited from the air, in which he supposed it *to have been contained*: to the mind of Cavendish, deeply meditating what might be the form of matter into which the *lost airs* could have been *condensed*, this inaccurate experiment immediately suggested the

have presumed to insult men of such *pure minds* as David Hume and Adam Smith, rudely *refusing to bear them company, but for an instant.*" (Life of Johnson, p. 22.) It is curious to compare this with Johnson's own account: "I was but once in Hume's company; and then his only attempt at merriment consisted in his display of a drawing too indecently gross to have delighted even in a brothel." (Hawkins.) The *real man* from whom Johnson turned on his heel, was one who added to the moral *purity* of the school of Voltaire the garb of an *ecclesiastic*,—a circumstance which perhaps may abate something of Lord Brougham's indignation at the ill-manners of Johnson.

light for which he was watching. Such is his own description of the manner of the discovery; and the course of experiments recorded in his note-book leave no shadow of a doubt that he has described it with truth.

In the progress of these experiments, that is to say, in his *fourth* experiment of exploding the gases, in an apparatus like Warltire's but *really close*, on the 5th day of July 1781, Cavendish arrived at the *exact volume of hydrogen* which destroyed in combustion the whole of *its own elasticity*, with the whole elasticity of the *exact volume of oxygen* found to exist in the common air with which he exploded it: in the same experiment he ascertained that the vessel, which was of such capacity as to hold 24,000 grains of water, had lost scarcely one-fifth of a grain in weight: he then varied his apparatus in such a manner as to collect a sufficient quantity of the liquid formed in these experiments, for chemical examination, and before the end of the month demonstrated it to be *pure water*.

And now point out to me, if you can, in the whole range of experimental science, *three facts* the ascertainment of which was more *obviously and indubitably conclusive* of the point in question. Is there any alternative left for scepticism? The total weight undiminished—three volumes of elastic matter gone—in its place pure water—could *any man* draw *any conclusion* from such experiments but *one*? could anything but *one foregone conclusion* have led a man to institute such a course of experiment? Does *the man* who has instituted, and made, such experiments, want any one to come to him two years afterwards with an idle *doctrine*, or *hypothesis*, as you call it, about the connection of water with some *undefined kind of phlogiston*, by way of explaining to him his own investigation? What is the use of a doctrine, or a hypothesis *after an inductive demonstration*, even if the hypothesis had had any real substance or distinctness of meaning? Are we to deny the author of the demonstration the credit of understanding it, for no better reason than that in the private notes of his chain of proofs we find no shout of *εῦρηκα*?

I omit here all the multiplied precautions to ensure the most perfect accuracy in regard to every elementary material

of these experiments—I omit the singular caution and sagacity which, on the unexpected intrusion of a minute quantity of nitric acid in one of his varied trials, induced Cavendish to wait till he had obtained evidence that *this* was the product of the *other ingredient* in atmospheric air, before he would publish his experiments: I put the question in a shape so simple that any one who has learnt but the language of chemistry may understand it; and I ask you once more,—ought you not, with all this, clearly stated, before you, to feel some compunction for having admitted a suspicion of the good faith of Cavendish, or made a question of his having been the sole discoverer?

Again,— I have shown you, that though these experiments were communicated to Priestley as soon as they were made, and by Priestley mentioned to the public in express terms as—“*Mr. Cavendish's experiment on the re-conversion of air into water;*” Priestley understood them no better than the communication which I have before mentioned of the discovery of nitrogen, and subsequently, with the aid of Watt's opinion, concluded that “water by exposing it to heat in porous earthen vessels is capable of being converted into *respirable air* by the influence of heat:” I have shown you out of that very letter of Watt, communicated to the Royal Society, on which the only real question rests—whether he understood the consequences of Cavendish's experiments nearly two years after they were finished—that Watt's *doctrine* about water and *phlogiston* was built on *this false supposition*, and that he adhered to it after Priestley had communicated to him *that experiment* which was designed to be a repetition of Cavendish's\*: I have shown you that in Priestley's repetition the inflammable gas which he used cannot have contained more than one-fifteenth of its weight of hydrogen, and if it had proved anything, would have proved that water consists chiefly of *carbon*†: lastly, I have shown you that both Priestley and Watt were entirely ignorant of the distinction between *hydrogen* and the *inflammable gases on which they experimented and reasoned*; and until at a later time they were taught that distinction by Cavendish, and thus learnt what the *real basis* of water is—

\* Report of the British Association, Postscript to Address, p. 24.

† Ibid. p. 27.

were obviously as incompetent to *understand*, as to *discover* its composition\*.

With these things before the world, you even now venture to reiterate, as your final conclusion, this most unjustifiable judgement—"It is undeniable that from less elaborate experiments Mr. Watt had before Cavendish drawn the inference then so startling, that it required all the boldness of the philosophic character to venture upon it,—the inference that water was not a simple element, but a combination of oxygen with *inflammable air thence called hydrogen gas*. That Mr. Watt first generalized the facts so as to arrive at this great truth, I think has been proved as clearly as any position in the history of physical science. It is equally certain from the examination of Mr. Cavendish's papers, and from the publication lately made of his journals, first, that he never so clearly as Mr. Watt drew the inference from his experiments; and secondly, that though those experiments were made before Mr. Watt's inferences, yet Mr. Cavendish's conclusion was not drawn privately even by himself till after Mr. Watt's inference had been made known to many others."—!!!

What friend of yours, my dear Lord, but must regret to see a great man trifling with his own reputation by thus confidently dealing with subjects of which he betrays so defective a knowledge? I sincerely lament, for my own part, that having once been honoured by your regard, and having always respected your talents, it should have fallen to me to presume in this manner to rectify your misapprehensions. I declined to enter into controversy with *you*, partly for old acquaintance sake, and partly because I thought you on this question less *responsible* than the official writer of the Institute of France. But you *would* do battle with me; and your weapons were none of the fairest: for instead of replying to my arguments, you did me the injustice, without provocation, to compare the abilities and character of the obscurest lover of science in England with those of one of the most eminent of its cultivators in France. I know not that I shall even now have convinced you that the meanest of our philosophical chemists, in his own art, and in a just cause, may be more than a match for the most

\* Report of the British Association, Postscript to Address, p. 25.

learned judge of *Patents*, or even for the ablest member of the “*Institut*,” whose studies have lain in another direction. A judge in a patent cause may see his way well enough, no doubt, through intricate scientific questions, if he is but prudent in his selection of authorities: but I do not perceive that in this case you have abided by any authority better than *your own in 1803*\*. You are even bold enough, on the strength of such authority, to differ from a deceased Secretary of the “*Institut*” itself, than whom few men were better acquainted with sciences not peculiarly his own: but though the subject is chemistry, though *you* have attended Black’s lectures, and though Black’s own discoveries are in question, I greatly fear that on almost every point in which you differ from Cuvier you are yourself in the wrong.

Thus, you are certainly wrong in *denying* Cuvier’s assertion—that permanently elastic fluids were *measured* by Hales; and you have only to consult the ‘*Analysis of the Air*,’ to be convinced of your mistake.

Again, you are wrong in *denying* Cuvier’s assertion, that “no one before Cavendish had distinguished fixed air as a separate *aërisform* substance:” and you need only look at Black’s treatise to assure yourself that he declined to decide, for want of evidence, whether it was an *aërisform* substance, or not; and left it among the class of—“bodies of which it is difficult to say, whether they are really *combined* with the *aërial* particles, or are merely *suspended* in the fluid, in consequence of their being of the *same specific gravity*†.”

\* “I first stated that opinion in a published form in 1803-04, Edinburgh Review, vol. iii.”—*Life of Lavoisier*, p. 253.

† *Cavallo on Air*, p. 361. 1781. Hawksbee approached the nearest to the discovery of the different density of gases, as early as 1707;—“whether,” said he, “the space deserted by the water [after an explosion of gunpowder in a close vessel] is possessed by a body of *the same weight and density*, or is of the same quality, as common air, I dare not determine; *since an experiment I have lately made seems to conclude it otherwise*.” He observed likewise “a loss, or absorption of this air, after it had reached its former temperature;” and suggested that a temporary distension of the springs or constituent parts, of the ambient air, as well as of those contained in the body of the gunpowder, may account for “*this odd phænomenon*.”—*Phil. Trans.*, vol. xxv. p. 2409.

But above all, you are *most wrong* in reprehending the former Secretary of the Institute, for “*making no mention whatever of Watt in connection with the discovery of the composition of water*”—for not confounding, that is, the rights of discovery—for not falsifying the history of chemistry in one of its most material parts—for not representing Watt as the claimant of a merit to which he had not the smallest pretensions—and thus degrading, with intent to exalt, the venerable name of one who has entitled himself to the admiring gratitude of ages, by realizing, beyond any other man, the vision which Bacon saw—of experimental *works of fruit*.

You have no sufficient ground, I think, for imputing to—“a person of M. Cuvier’s eminent attainments, filling the high office of ‘*Secrétaire perpétuel*,’ and charged with the delicate and important duty of recording the history of science yearly”—that “he has not read Mr. Cavendish’s paper\*, or Dr. Black’s treatise.” And certainly you have no ground to “lament,” with respect to him, “that the history of science should be written with such *remarkable carelessness* and such *manifest inattention to the facts*,”—however true it may be, were the censure justly pointed—“that to find mistakes so very gross in the works of ordinary writers might excite little surprise; but when they are embodied in the history of the *National Institute*, and when they come to us under the name, among

\* One of Lord Brougham’s reasons for thinking that Cuvier had never read Cavendish’s paper is, that he says,—“Cavendish unfolded his discoveries in a manner even more striking than the discoveries themselves”—an assertion which will scarcely be disputed by any competent judge who compares the brief perspicuity of expression, and the select sequence of most exact experiment, which shines in every page of Cavendish, with the rambling, inconsequent manner of thinking and writing, general in his time, and I fear not infrequent in our own. Lord Brougham also accuses Cuvier of stating, that Cavendish established in his paper of 1764 these propositions—“*l’eau n’est pas un élément ; il existe plusieurs sortes d’air essentiellement différentes.*” This statement, with the exception of the date, to which it is applicable only in part, I have shown to be correct : but if it stands in connexion with the date of 1764, as represented by Lord Brougham, it is not improbable that the word ‘*l’eau*’, in this paragraph, is a misprint for ‘*l’air*.’

the very first in all sciences, of Cuvier, we may at once wonder and mourn\*."

I only trust that the stone which you have so rashly cast at Cuvier will not recoil on any other head. I still trust sincerely that so severe a reproof will not *permanently* rest on the *present* "Perpetual Secretary" of the Institute of France; and that conformably to the known manliness of his character, and clearness of his understanding, M. Arago will yet rectify, as he knows how, the inadvertence into which he has fallen.

It now only remains for me to remark on your last words in reply to one who has supported with far greater ability than myself the same opinions which I have expressed.

I have known you, my dear Lord, more strenuously and skilfully employed than in deciding these questions for chemists; and think I remember it to have been one of the arts of a dexterous *advocate*, with which you were then familiar, to speak somewhat *largely* in an opening speech, of evidence which yet it might not be discreet to bring into court: and so I suppose it is now; for in animadverting upon the ignorance of this enemy in ambush, whom however you seem to suspect of being no ordinary man, I perceive you affirm, that you "have lying before you *fifteen pages* of statements of chemical errors in the thirty-four pages of his paper, and as these corrections are the work of *a most experienced, learned and practical chemist* whom you consulted, you have entire reliance on his report and opinion." It was some disappointment to me, at first, to find that you kept the *fifteen pages* in your pocket; but I remembered, how it happened not unfrequently of old, that in the torrent of that forensic eloquence which so often dazzled and delighted your hearers, something that should have been kept back would occasionally slip out, of which an astute adversary did not fail to make his advantage. And even so it is still: from the *fifteen* critical pages you have allowed *one* criticism to creep out, as too good to be suppressed. And here it is:—

"I leave him" (the author of this heap of errors) "in the

\* Brougham's Lives, vol. ii. p. 507.

hands of M. Arago, who will observe with some wonder that he has been accused, and judged, and condemned, by a chemist so well-versed in that science, and so reflecting, as to announce the astonishing novelty—that the exhibition of sulphur to sulphuric acid reduces that acid, and restores it to its primitive state of sulphur! The writer had probably read somewhere that sulphuric acid is reduced to sulphurous by the process; for he is assuredly the first that had ever hit upon the acid's reduction by sulphur to 'its primitive state' \*."

Now we will at least give credit to the *present* perpetual Secretary of the Institute, to whose scorn you devote the unhappy Reviewer, for having read the papers of Cavendish; and he would no doubt recollect this remarkable passage in the "experiments on factitious airs" (1764) to which the Reviewer should seem to be referring—"Sulphur is allowed by chemists to consist of the plain vitriolic acid united to phlogiston; the volatile sulphureous acid appears to consist of the same acid united to a less proportion of phlogiston than what is required to form sulphur; a circumstance which I think shows the truth of this is, that if oil of vitriol be distilled from sulphur, the liquor which comes over will be the volatile sulphureous acid." M. Arago might perhaps compare these early notions of Cavendish with the Reviewer's account of the phlogistic opinions, not in your interpolated words, but in his own—"It was concluded therefore that it was the *same* phlogiston which was derived from all those substances (charcoal, sugar, metallic bodies, &c.), however different in their nature: a similar succession of phænomena is presented by sulphur: if it be burnt, it forms sulphuric acid; but if the acid thus formed be heated with *phosphorus*, or *charcoal*, or *sugar*, or even *sulphur* itself, it is equally restored to its primitive state†,"—and having read this account, supposing M. Arago for a moment to be only as experienced, as learned, and as practical a chemist, as he whom you have consulted out of court, and no more—supposing him, that is, to believe, with your anonymous friend and yourself, that the total reduction of sulphuric acid by sulphur is a laughable absurdity—M. Arago would yet see,

\* Brougham's Lives, vol. ii. p. 511.

† Quarterly Review, Dec. 1845, p. 106.

that there is nothing laughable, or ignorant, in the statement of the Reviewer; though in strictness of language it might have been more correct to say, that—‘sulphuric acid heated with *phosphorus*, or *charcoal*, or *sugar*, is reduced to its *primitive* state, and even heated with *sulphur* is reduced to its *previous* state of sulphurous acid’ :—and I think M. Arago might *wonder* a little at finding how much you, and your learned, experienced, and practical friend, would make of a mere *verbal* slip.

But *if*, as I am apt to suspect, the *perpetual Secretary* is a *better* chemist than yourself, or knows better, this time at least, on whose *information* to rely, then he will whisper to you privately, that sulphuric acid *really is reduced*, astonishing novelty as it seems to you, even by *sulphur itself*; and he will doubtless proceed to explain to you how this marvel comes to pass: he will remind you, that the great chemist of our time, whose life you have written, when attempting to decompose sulphur, found it so closely united with a very considerable quantity of hydrogen, that he remained for some time in the belief, that he had effected its decomposition; and M. Arago will add, that since *hydrogen* decomposes *sulphurous acid*, it follows, that sulphuric acid cannot but be reduced by sulphur, in all the forms in which sulphur is commonly experimented with, to its *primitive* state, and that the Reviewer therefore is literally right.

And now, retaining the very sincere respect which I have always felt for one who has so laudably devoted the leisure hours of a busy life of public service to the promotion of literature and science, I hope I may have persuaded you, that it is at once more safe, and more just, for those who have not had leisure to pursue chemical studies to their foundation, to leave chemistry and chemists to themselves—at least so far as regards the *minutiæ* of the science, and arbitrations of the rights of discovery: and I take the license of old acquaintance to advise, that if you *will* venture on such dangerous ground, you should at least learn how to choose your authorities; and when you find even Robison, and Watt, deserting you, and the *perpetual Secretary* so tardy in coming to the rescue, you should not think it enough to reflect that in 1803—1839—1845

and 1846—you yourself stated and re-stated an opinion contrary to the public\* voice of the chemists of England.

I have the honour to remain, with undiminished regard,  
My dear Lord, your faithful Servant,

W. VERNON HARCOURT.

• Lord Brougham expresses more surprise than is just, that I take no notice of his having *quoted* a private letter to the son of Mr. Watt on the subject of his father's claims. I am aware that Lord Brougham says he has seen such a letter, and says also that the opinion expressed in it respecting Watt's MSS. is different from the opinion attributed to Dr. Henry by me: but I am not aware that Lord Brougham has given *any quotation* from this letter; nor if he had, would any *partial quotation* have satisfied me, that Dr. Henry's opinion was at any time different from that which he expressed to me, when I mentioned to him the sentiments which I had heard fall from M. Arago concerning the MSS. at Aston, and the insincerity of Cavendish. Dr. Henry then said, that he had seen nothing in those MSS. either to justify that impression, or to alter the received opinion respecting the discovery of the composition of water. Who indeed can doubt but that the MSS., had they contained any evidence to support an object which has been so long urged by private solicitation, would have been made public long ago?

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POSTSCRIPT.

*To Richard Taylor, Esq.*

DEAR SIR,

Having in a contribution to the last volume of the Philosophical Magazine touched on certain views of the *evidence* of inductive philosophy, which I apprehend to be erroneous, I wish, with your permission, to explain in a somewhat fuller degree, though briefly, my own conceptions of the real nature of that evidence; and request you to subjoin these explanations as a supplementary note, either in the Magazine or in the *separate* publication, which you have been so kind as to propose, of my account of the discovery of the gases and of the composition of water.

I remain, dear Sir, Yours faithfully,  
Bolton Percy, June 15. W. VERNON HARCOURT.

The idea that the force of inductive evidence† depends on the *number* of instances, has obtained currency in some of

† See Phil. Mag., vol. xxviii. p. 513.

our academical schools, chiefly, I believe, from the authority of a great moral and metaphysical writer, whose studies did not lie in the direction of natural and experimental philosophy. "A low presumption," says Bishop Butler, "often repeated, will amount to a moral certainty. Thus a man's having observed the ebb and flow of the tide today, affords some sort of presumption, though the lowest imaginable, that it may happen again tomorrow: but the observation of this event for so many days and months and ages together, as it has been observed by mankind, gives us a full assurance that it will\*."

It is the greatest of Butler's merits, that on subjects of the most abstruse and important speculation, formerly discussed by *à priori* reasoning, he substituted the inductive for the hypothetical method, and argued with just precaution and due corrections from what *is* to what *probably, or not improbably, may be*. "Into the nature," however, "the foundation and measure of probability, it is not my design," he adds, "to inquire further; this belongs to the subject of logic, and is a part of that subject which has not yet been fully considered." Nevertheless since the soundness of all our practical judgements, and all our intellectual conclusions, depends on our understanding well the grounds whereon they rest, it is of the utmost consequence to disentangle, as far as we can, the knot into which three distinct questions—namely the investigation of the laws of nature, the prospect of their continuance, and the degrees of certainty and presumption on which we judge, and on which we act—are bound and mixed up together in the passages now referred to. Let us then take the principles of evidence here assumed by Butler, and subject them to a strict analysis.

For this purpose we will vary the *subject* of his illustration. We will suppose a man to have observed, instead of the ebb and flow of the tide, a flash of lightning, or the fall of a stone from the air. Shall we say that the having observed one of these phænomena today affords some sort of presumption, though the lowest imaginable, that it may happen again tomorrow? On the supposition that we are in the dark concerning the causes and circumstances which determine these

\* Butler's *Analogy*, Introduction, p. 1.

events, does not the doctrine of chances teach us, on the contrary, that there are an infinite number of presumptions to one, that the same event will not happen again tomorrow, or on any other day that can be named? The solitary event does indeed suggest to us, not "the lowest imaginable presumption," but the highest possible certainty, that there are causes in operation which *may* at any time produce the same result; but apart from *the suspicion of some permanent cause*, it does not afford a shadow of a presumption that the same event *will* happen again at all.

Dismissing then the notion that an inductive certainty is a bundle of low presumptions from single facts, let us consider what the nature of the evidence really is which is furnished by the repetition of an event.

1. If a die, presumed to be on all sides of uniform weight, be thrown, there is no presumption that the number which has come up on the first throw will come up again on the next, the presumptions being on the contrary as many against *that* as against any one of the numbers marked on the die. If however in *several successive throws* the same number *does* come up, a presumption *does* presently arise, and increases rapidly with the repetition of the throws, that the same number will continue to come up. The origin of this presumption is obvious. The violation of the *indifference* of the chances has indicated a definite cause, which is conceived to determine the result of the throw.

2. If I have seen the crater of a volcano constantly smoking for fifty years, I shall entertain some expectation of its smoking tomorrow: but the presumption that it will smoke tomorrow, or that it will continue to smoke for fifty years to come, is very far from being as *violent* as the presumption that a die which I have thrown fifty times with the like result will continue to show the same face fifty times more.

The distinction between the two cases is evidently no other than this. In the latter case I have inferred an *invariable* cause, namely that of weight, as determining the event; in the former I have inferred that causes are operating, which I presume from the frequent recurrence of the event to operate with *some degree of permanence*, but of which the duration, till I can

trace the laws and circumstances which govern the eruptions of volcanoes, is so doubtful and incalculable, that the cessation of the event is almost as likely as its continuance.

The ebb and flow of the tide, the rising and setting of the sun, suggest the same kind of thoughts as the determinate fall of the die: only in these cases the *generality* of the phænomena rather than their constancy, or a precise apprehension of their cause, guides the mind to its conclusion that *here is a law of nature*—a conclusion which once arrived at, from however few observations, produces a probability that the event will continue, equal to the observation of ages.

3. But the amount of rational probability is carried much further, when the mind rises to the consideration of a geometrical path for the earth, or a graduated attraction between the earth and sun, and has ascertained those precise and universal laws which regulate the movements of the heavenly bodies. By whatever means this knowledge is obtained, the assurance of the future event is raised by it to the highest pitch, and *that* often after very few observations indeed; as any one will see, who considers the great degree of certainty which a few sights of a comet, if we abstract the chance of disturbing forces, can give of its periodical return.

In all these cases it is evident that the real medium through which we connect the *actual* with the *future* is the apprehension of an *efficient*, and if we would probe the subject to the bottom, an *intelligent cause*. This is the principle, issuing from the inward analogies of our own minds, which lies at the foundation of philosophy, and gives force to inductive evidence; this is the principle which turns sequences into effects, gathers individual facts into laws, and connects, so far as it is possible to connect, the past, the present, and the future.

Those who clearly perceive the truth here stated, will see that there is not the least ground for considering, with a certain school of metaphysicians, our expectation of an event like the rising of the sun as an *ultimate fact* in the constitution of our minds, or for resolving a reasonable belief in the *continuance of the laws of nature* into an instinctive and implicit credulity.

What the instinctive process may be by which irrational

beings are led to pursue a course of conduct conformable to that which in those endowed with reason proceeds from reasonable inference, is another question. It is also another question, what is the difference in the degrees of certainty required to satisfy the mind of the philosopher, and to regulate the conduct of the man. It is sufficiently plain that a wise man will not build his house on the edge of a volcano, however low the presumption may be that its eruptions will last.

The idea that there is any absolute certainty of *future* physical events rests on no grounds of reason. Mathematics have an abstract and absolute certainty of their own. The certainty of physics is absolute only as respects the *actual* existence of facts and laws. Our views in regard to the future are necessarily, in natural knowledge, qualified and conditional, for the highest no less than the lowest of the presumptions which it contemplates. We believe that the sun rises and the tide flows, as the volcano smokes, in conformity with laws whether simple or complex, known or unknown, which will not lightly be changed. Among the causes of these effects, no rational philosopher ever overlooked the FIRST: and since we cannot calculate the course of His secret operations, we must be content to allow that however great the difference may be in the value of physical presumptions as establishing actual laws, there is no such thing as physical certainty for *the time to come*.

The certainties of the *actual* laws of nature are attained to by inductive observation. Where there is any regularity discernible in events, a few observations, on the principle above explained, indicate a cause. Then the business of the inductive philosopher is to investigate that cause, not by repeating the observation, from which he would gain neither light nor certainty, but by varying it till the possible causes of the event, by the process of sifting out the inconstant circumstances, are reduced to the simplest and most constant conditions. Even when for accuracy observations appear to be repeated, the value of the repetition consists in the presumed variation of unknown circumstances, by means of which the accidental errors of disturbing causes destroy one another.

In *experiments*, where the circumstances are diversified at

our will, and we proceed, in the language of Bacon, to bind the Proteus, and force nature to deliver her oracles, the more dexterous and accurate the experimenter, the less need he has either to repeat, or vary, his experiments. The difference between a *learned* and an *unlearned* experimentalist, the advantage which a disciple of Bacon and Newton, and Black and Cavendish and Lavoisier, possesses over men uninstructed in the science of induction, is—that the former has learnt the *art* of cross-examining nature, and the latter has not. The difference between the man who has a *genius* for inductive philosophy, and the man who has none, is—that the one has a *sagacity* which the other wants, in discovering *media* of proof, and driving his interrogatories *to a point*. Little or nothing depends on the multiplication of experiments, everything on the selection; and the only guides to selection are, first, a quick analogical perception—and, secondly, a just and sound appreciation—of the *causes* of phænomena.

## APPENDIX.

*To Richard Taylor, Esq.*

DEAR SIR,

The letter to Oldenburg, of which I gave an account in one of your former Numbers, as containing the first draught of Newton's speculations respecting an ætherial medium, certainly deserves more notice than it has obtained. It has scarcely, I think, been quoted, except by Dr. Young; and its existence is but little known, even among the best-informed scientific men, notwithstanding its publication (with some verbal inaccuracies) by Dr. Birch in his History of the Royal Society. Under these circumstances, I have no doubt that a reprint of it would be very acceptable as a curious matter of history, and especially as tending to throw light on the character of Newton's mind.

The letter to Boyle is more accessible; but the fact of its containing the earliest attempt, as I have pointed out in my letter to Lord Brougham, to explain the nature of gaseous substances, has failed to attract the attention which it merits at the hands of the historians of chemistry.

I remain, dear Sir,

Yours faithfully,

Bolton Percy, August.

WILLIAM VERNON HARCOURT.

Mr. Isaac Newton's *Letter, Hypothesis, Observations and Experiments touching his Theory of Light and Colours; in confirmation and illustration of his former discourse on the same subject*\*.

SIR,

I have sent you the papers I mentioned by John Stiles. Upon reviewing them, I find some things so obscure as

\* Read before the Royal Society, December 9, 1675. and some following days of their meeting.

might have deserved a further explanation by schemes; and some other things I guess will not be new to you, though almost all was new to me when I wrote them. But as they are, I hope you will accept of them, though not worth the ample thanks you sent. I remember in some discourse with Mr. Hook, I happened to say that I thought light was reflected, not by the parts of glass, water, air, or other sensible bodies, but by the same confine or superficies of the æthereal mediums which refracts it, the rays finding some difficulty to get through it, in passing out of the denser into the rarer medium, and a greater difficulty in passing out of the rarer into the denser; and so, being either refracted or reflected by that superficies as the circumstances they happened to be in at their incidence, make them able, or unable, to get through it. And for confirmation of this, I said further, that I thought the reflexion of light at its tending out of glass into air, would not be diminished or weakened by drawing away the air in an air-pump, as it ought to be, if they were the parts of air that reflected; and added that I had not tried this experiment, but thought he was not unacquainted with notions of this kind. To which he replied, that the notion was new, and he would, the first opportunity, try the experiment I propounded. But, upon reviewing the papers I send you, I found it there set down for tried, which makes me recollect that about the time I was writing those papers, I had occasionally observed in an air-pump here, at Christ's College, that I could not perceive the reflexion of the inside of the glass diminished in drawing out the air. This I thought fit to mention, lest my former forgetfulness, through having long laid aside my thoughts on these things, should make me seem to have set down for certain what I never tried.

Sir, I had formerly purposed never to write any hypothesis of light and colours, fearing it might be a means to engage me in vain disputes; but I hope a declared resolution to answer nothing that looks like a controversy (unless possibly at my own time upon some other by-occasion) may defend me from that fear. And therefore considering that such an hypothesis would much illustrate the papers I promised to send you, and having a little time this last week to spare, I have not

scrupled to describe one so far as I could on a sudden recollect my thoughts about it, not concerning myself whether it be thought probable or improbable, so it do but render the papers I send you, and others sent formerly, more intelligible. You may see, by the scratching and interlining, it was done in haste, and I have not had time to get it transcribed, which makes me say I reserve the liberty of adding or altering it, and desire that you would return those and the other papers when you have done with them. I doubt there is too much to be read at one time, but you will soon know how to order that. At the end of the hypothesis you'll see a paragraph to be inserted, as is there directed. I should have added another or two, but I had not time, but such as it is, I hope you will accept it.

Sir, I am,

Your humble Servant,

I. NEWTON.

Mr. I. Newton's *Letter sent to H. O.* \*

*An Hypothesis explaining the Properties of Light discoursed of in my several papers.*

SIR,

In my answer to Mr. Hook you may remember I had occasion to say something of hypotheses, where I gave a reason why all allowable hypotheses in their genuine constitution should be conformable to my theories, and said of Mr. Hook's hypothesis, that I took the most free and natural application of it to phænomena to be this:—"That the agitated parts of bodies, according to their several sizes, figure and motions, do excite vibrations in the æther of various depths or bignesses, which being promiscuously propagated through that medium to our eyes, effect in us a sensation of light of a white colour; but if by any means those of unequal bignesses be separated from one another, the largest beget a sen-

\* Registry Book of the Royal Society, vol. v., from 1675 to 1679. See also Birch's History of the Royal Society, vol. iii. p. 248.

sation of a red colour, the least or shortest of a deep violet, and the intermediate ones of intermediate colours, much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air of various bignesses, which according to those bignesses make several tones in sound, &c.\*" I was glad to understand, as I apprehended from Mr. Hook's discourse at my last being at one of your assemblies, that he had changed his former notion of all colours being compounded of only two original ones, made by the two sides of an oblique pulse, and accommodated his hypothesis to this my suggestion of colours, like sounds, being various, according to the various bigness of the pulses. For this I take to be a more plausible hypothesis than any other described by former authors; because I see not how the colours of thin transparent plates, or skins, can be handsomely explained without having recourse to ætherial pulses. But yet I like another hypothesis better, which I had occasion to hint something of in the same letter in these words:—"The hypothesis of light's being a body, had I propounded it, has a much greater affinity with the objector's own hypothesis than he seems to be aware of, the vibrations of the æther being as useful and necessary in this as in his. For assuming the rays of light to be small bodies emitted every way from shining substances, those, when they impinge on any refracting or reflecting superficies, must as necessarily excite vibrations in the æther as stones do in water when thrown into it. And supposing these vibrations to be of several depths or thicknesses, accordingly as they are excited by the said corpuscular rays of various sizes and velocities, of what use they will be for explicating the manner of reflexion and refraction, the production of heat by the sun-beams, the emission of light from burning, putrifying, or other substances whose parts are vehemently agitated, the phænomena of thin transparent plates and bubbles, and of all natural bodies, the manner of vision, and the difference of colours, as also their harmony and discord, I shall leave to their consideration who may think it worth their endeavour to apply this hypothesis to

\* Letter to O. Camb. July 11, 1672. Phil. Trans. No. 88. p. 5087-8.

the solution of phænomena." Were I to assume an hypothesis, it should be this, if propounded more generally so as not to determine what light is, further than that it is something or other capable of exciting vibrations in the æther; for thus it will become so general and comprehensive of other hypotheses as to leave little room for new ones to be invented; and therefore because I have observed the heads of some great virtuosos to run much upon hypotheses, as if my discourses wanted an hypothesis to explain them by, and found that some, when I could not make them take my meaning when I spake of the nature of light and colours abstractedly, have readily apprehended it when I illustrated my discourse by an hypothesis; for this reason I have here thought fit to send you a description of the circumstances of this hypothesis, as much tending to the illustration of the papers I herewith send you; and though I shall not assume either this or any other hypothesis, not thinking it necessary to concern myself whether the properties of light discovered by me be explained by this, or Mr. Hook's, or any other hypothesis capable of explaining them, yet while I am describing this, I shall sometimes, to avoid circumlocution and to represent it more conveniently, speak of it as if I assumed it and propounded it to be believed. This I thought fit to express, that no man may confound this with my other discourses, or measure the certainty of one by the other, or think me obliged to answer objections against this script; for I desire to decline being involved in such troublesome, insignificant disputes.

But to proceed to the hypothesis:—1. It is to be supposed therein, that there is an ætherial medium, much of the same constitution with air, but far rarer, subtler, and more strongly elastic. Of the existence of this medium, the motion of a pendulum in a glass exhausted of air almost as quickly as in the open air is no inconsiderable argument\*. But it is not to be

\* Either we must presume that a word is here omitted in the manuscript, and that the sentence should stand thus—"the motion of a pendulum '*ceasing*' in a glass exhausted of air almost as quickly as in the open air," and suppose this statement made on the authority of Boyle's experiments, in which the difference of the times of vibration in the two cases was "scarce sensible" (New Exp. Phys.-Mech., Exp. 26),—or we must conclude

supposed that this medium is one uniform matter, but composed partly of the main phlegmatic body of æther, partly of other various ætherial spirits, much after the manner that air is compounded of the phlegmatic body of air intermixed with various vapours and exhalations. For the electric and magnetic effluvia, and the gravitating principle, seem to argue such variety. Perhaps the whole frame of nature may be nothing but various contextures of some certain ætherial spirits or vapours, condensed as it were by precipitation, much after the manner that vapours are condensed into water, or exhalations into grosser substances, though not so easily condensable; and after condensation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, which, by virtue of the command, increase and multiply, became a complete imitator of the copy set her by the Protoplasm. Thus perhaps may all things be originated from æther.

At least the electric effluvia seem to instruct us that there is something of an ætherial nature condensed in bodies. I

that Newton had already, in 1675, arrived, by experiments of his own, at the same conclusion to which Derham and Hawksbee came in 1704, that in consequence of the extension of the arcs of vibration, the vibrations, though quicker in their rates, are in their times "slower in the exhausted than in the unexhausted receiver" (Phil. Trans. No. 294. p. 1785). In the 6th Section of the 2nd Book of the *Principia*, in the scholium to the 31st Prop., Newton has given the results of an experiment made by him for the purpose of determining whether the vibration of bodies affords any indication of a resisting medium as present in their internal pores, independent of the resistance which the air makes to the movement of their surfaces. The principal object of this experiment seems to have been, to demonstrate that there exists no resistance of this description equal to that which Descartes's theory of a *plenum* would require. Newton found the resistance of the internal parts of the box with which he made the experiment at least more than 5000 times less than that of its surface. A box, first empty, and then loaded with metal, was swung by a thread of 11 feet long; the resistance of the full box compared with that of the empty, by the ratio of the weights to the number of oscillations within measured distances, was found to be in a proportion not greater than that of 78 to 77. The circumstances of the experiment, however, did not admit of such accuracy as to carry its import beyond the negative object for which it was instituted; and Newton, in the reasons which he has subsequently assigned for admitting the existence of an æther, made no use of this.—W. V. H.

have sometimes laid upon a table a round piece of glass about two inches broad, set in a brass ring, so that the glass might be about one-eighth or one-sixth of an inch from the table, and the air between them inclosed on all sides by the ring, after the manner as if I had whelmed a little sieve upon the table. And then rubbing a pretty while the glass briskly with some rough and raking stuff, till some very little fragments of very thin paper laid on the table under the glass began to be attracted and move nimbly to and fro; after I had done rubbing the glass, the papers would continue a pretty while in various motions, sometimes leaping up to the glass and resting there awhile, then leaping down and resting there, then leaping up, and perhaps down and up again, and this sometimes in lines seeming perpendicular to the table, sometimes in oblique ones; sometimes also they would leap up in one arch and down in another divers times together, without sensible resting between; sometimes skip in a bow from one part of the glass to another without touching the table, and sometimes hang by a corner and turn often about very nimbly, as if they had been carried about in the midst of a whirlwind, and be otherwise variously moved,—every paper with a divers motion. And upon sliding my finger on the upper side of the glass, though neither the glass nor the enclosed air below were moved thereby, yet would the papers as they hung under the glass receive some new motion, inclining this way or that way, accordingly as I moved my finger. Now whence all these irregular motions should spring I cannot imagine, unless from some kind of subtle matter lying condensed in the glass, and rarefied by rubbing, as water is rarefied into vapour by heat, and in that rarefaction diffused through the space round the glass to a great distance, and made to move and circulate variously, and accordingly to actuate the papers, till it returns into the glass again, and be reconensed there. And as this condensed matter by rarefaction into an ætherial wind (for by its easy penetrating and circulating through glass I esteem it ætherial) may cause these odd motions, and by condensing again may cause electrical attraction with its returning to the glass to succeed in the place of what is there continually reconensed; so may the gravitating attraction of the earth be

caused by the continual condensation of some other such like aetherial spirit, not of the main body of phlegmatic aether, but of something very thinly and subtilely diffused through it, perhaps of an unctuous, or gummy tenacious and springy nature; and bearing much the same relation to aether which the vital aërial spirit requisite for the conservation of flame and vital motions does to air. For if such an aetherial spirit may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter for the continual uses of nature (adhering to the sides of those pores after the manner that vapours condense on the sides of a vessel), the vast body of the earth, which may be everywhere to the very centre in perpetual working, may continually condense so much of this spirit as to cause it from above to descend with great celerity for a supply: in which descent it may bear down with it the bodies it pervades with force proportional to the superficies of all their parts it acts upon, nature making a circulation by the slow ascent of as much matter out of the bowels of the earth in an aërial form, which for a time constitutes the atmosphere, but being continually buoyed up by the new air, exhalations, and vapours rising underneath, at length (some part of the vapours which return in rain excepted) vanishes again into the aetherial spaces, and there perhaps in time relents and is attenuated into its first principle. For nature is a perpetual circulatory worker, generating fluids out of solids, and solids out of fluids, fixed things out of volatile, and volatile out of fixed, subtile out of gross, and gross out of subtile, some things to ascend and make the upper terrestrial juices, rivers, and the atmosphere, and by consequence others to descend for a requital to the former. And as the earth, so perhaps may the sun imbibe this spirit copiously, to conserve his shining, and keep the planets from receding further from him: and they that will may also suppose that this spirit affords or carries with it thither the solary fuel and material principle of light, and that the vast aetherial spaces between us and the stars are for a sufficient repository for this food of the sun and planets. But this of the constitution of aetherial natures by the bye.

In the second place, it is to be supposed that the æther is a vibrating medium like air, only the vibrations far more swift and minute; those of air made by a man's ordinary voice, succeeding one another at more than half a foot or a foot distance, but those of æther at a less distance than the hundred-thousandth part of an inch. And as in air the vibrations are some larger than others, but yet all equally swift (for in a ring of bells the sound of every tone is heard at two or three miles distance in the same order that the bells are struck), so I suppose the ætherial vibrations differ in bigness, but not in swiftness. Now these vibrations, besides their use in reflexion and refraction, may be supposed the chief means by which the parts of fermenting or putrifying substances, fluid liquors, or melted, burning, or other hot bodies, continue in motion, are shaken asunder like a ship by waves, and dissipated into vapours, exhalations, or smoke, and light loosed or excited in those bodies, and consequently by which a body becomes a burning coal, and smoke flame; and I suppose flame is nothing but the particles of smoke turned by the access of light and heat to burning coals, little and innumerable.

Thirdly, the air can pervade the bores of small glass pipes, but yet not so easily as if they were wider, and therefore stands at a greater degree of rarity than in the free aërial spaces, and at so much greater a degree of rarity as the pipe is smaller, as is known by the rising of water in such pipes to a much greater height than the surface of the stagnating water into which they are dipped\*. So I suppose æther, though

\* The unprinted paper of "Observations" which accompanied this, and which was the first form of the "Optics," contained a passage on the present subject, omitted in that publication, which deserves to be quoted for the acknowledgment it contains of the merit of Hook. In the third prop. of the second book, Newton remarks:—"To the increase of the opacity of these bodies it conduces something, that by the 23rd observation, the reflexion of thin transparent substances are considerably stronger than those made by the same substances of a greater thickness." Here the paper subjoins,—"And to the reflexion of solid bodies it may be further added, that the interstices of their parts are void of air. For that for the most part they are so is reasonable to believe, considering the inaptitude which air hath to pervade small cavities, as appears by the ascension of water in slender glass pipes, paper, cloth, and other such like substances, whose pores

it pervades the pores of crystal, glass, water, and other natural bodies, yet it stands at a greater degree of rarity in those pores than in the free æthereal spaces, and at so much a greater degree of rarity as the pores of the body are smaller. Whence it may be that spirit of wine, for instance, though a lighter body, yet having subtler parts, and consequently smaller pores than water, is the more strongly refracting liquor. This also may be the principal cause of the cohesion of the parts of solids and fluids, of the springiness of glass and other bodies whose parts slide not one upon another in bending, and of the standing of the mercury in the Torricelian experiment, sometimes to the top of the glass, though a much greater height than twenty-nine inches. For the denser æther which surrounds these bodies must crowd and press their parts together, much after the manner that air surrounding two marbles presses them together if there be little or no air between them. Yea, and that puzzling problem, *by what means the muscles are contracted and dilated to cause animal motion, may receive greater light from hence than from any other means men have hitherto been thinking on.* For if there be any power in man to condense and dilate at will the æther that pervades the muscle, that condensation or dilatation must vary the compression of the muscle made by the ambient æther, and cause it to swell, or shrink, accordingly; for though common water will scarce shrink by compression and swell by relaxation, yet (so far as my observation reaches) spirit of wine and oil will; and Mr. Boyle's experiment of a tadpole shrinking very much by hard compressing the water in which it swam, is an argument that animal juices do the same: and

are found too small to be replenished with air, and yet large enough to admit water, and by the difficulty wherewith air pervades the pores of a bladder, through which water finds ready passage. And according to the 11th observation, the cavities thus void of air will cause the same kind of effect, as to reflexion, which those do that are replenished with it; but yet something more manifestly, because the medium in relation to refractions is rarest when most empty of air, as Mr. Hook hath proved in his *Micrographia*, in which book he hath also largely discoursed of this and the precedent proposition, and delivered many other very excellent things concerning the colour of thin plates, and other natural bodies, which I have not scrupled to make use of as far as they were for my purpose."—W. V. H.

as for their various pressure by the ambient æther, it is plain that that must be more or less, accordingly as there is more or less æther within to sustain and counterpoise the pressure of that without. If both æthers were equally dense, the muscle would be at liberty as if pressed by neither: if there were no æther within, the ambient would compress it with the whole force of its spring. If the æther within were twice as much dilated as that without, so as to have but half as much springiness, the ambient would have half the force of its springiness counterpoised thereby, and exercise but the other half upon the muscle; and so in all other cases the ambient compresses the muscle by the excess of the force of its springiness above that of the springiness of the included. To vary the compression of the muscle therefore, and so to swell and shrink it, there needs nothing but to change the consistence of the included æther; and a very little change may suffice, if the spring of æther be supposed very strong, as I take it to be many degrees stronger than that of air.

Now for the changing the consistence of the æther, some may be ready to grant that the soul may have an immediate power over the whole æther in any part of the body, to swell or shrink it at will; but then how depends the muscular motion on the nerves? Others therefore may be more apt to think it done by some certain ætherial spirit included within the *dura mater*, which the soul may have power to contract or dilate at will in any muscle, and so cause it to flow thither through the nerves; but still there is a difficulty why this force of the soul upon it does not take off the power of springiness, whereby it should sustain more or less the force of the outward æther. A third supposition may be, that the soul has a power to inspire any muscle with this spirit, by impelling it thither through the nerves; but this too has its difficulties; for it requires a forcible intruding the spring of the æther in the muscles by pressure exerted from the parts of the brain; and it is hard to conceive how so great force can be exercised amidst so tender matter as the brain is; and besides, why does not this ætherial spirit, being subtile enough, and urged with so great force, go away through the *dura mater* and skins of the muscle, or at least so much of the

other æther go out to make way for this which is crowded in? To take away these difficulties is a digression, but seeing the subject is a deserving one, I shall not stick to tell you how I think it may be done.

First, then, I suppose there *is* such a spirit; that is, that the animal spirits are neither like the liquor, vapour, or gas, of spirits of wine; but of an ætherial nature, subtile enough to pervade the animal juices as freely as the electric, or perhaps magnetic, effluvia do glass. And to know how the coats of the brain, nerves, and muscles, may become a convenient vessel to hold so subtile a spirit, you may consider how liquors and spirits are disposed to pervade, or not pervade, things on other accounts than their subtilty; water and oil pervade wood and stone, which quicksilver does not; and quicksilver, metals, which water and oil do not; water and acid spirits pervade salts, which oil and spirit of wine do not; and oil and spirit of wine pervade sulphur, which water and acid spirits do not; so some fluids (as oil and water), though their parts are in freedom enough to mix with one another, yet by some secret principle of *unsociableness* they keep asunder; and some that are *sociable* may become *unsociable* by adding a third thing to one of them, as water to spirit of wine by dissolving salt of tartar in it. The like *unsociableness* may be in ætherial natures, as perhaps between the aethers in the vortices of the sun and planets; and the reason why air stands rarer in the bores of small glass pipes, and æther in the pores of bodies, may be, not want of subtilty, but *sociableness*\*; and on this ground, if the ætherial vital spirit in a man be very *sociable* to the marrow and juices, and *unsociable* to the coats of the brain, nerves, and muscles, or to any thing lodged in the pores of those coats, it may be contained thereby, notwithstanding its subtilty; especially if we suppose no great violence done to it to squeeze it out, and that it may not be altogether so subtile as the main body of æther, though subtile enough to pervade readily the animal juices, and that as any of it is spent, it is continually supplied by new spirit from the heart.

\* In the third book of the Optics, Newton states more accurately the true theory of capillary attraction; but the *germ* of that theory is certainly contained in these expressions.—W. V. H.

In the next place, for knowing how this spirit may be used for animal motion, you may consider how some things unsociable are made sociable by the mediation of a third. Water, which will not dissolve copper, will do it if the copper be melted with sulphur. Aquafortis, which will not pervade gold, will do it by addition of a little sal-ammoniac or spirit of salt. Lead will not mix in melting with copper; but if a little tin, or antimony, be added, they mix readily, and part again of their own accord, if the antimony be wasted by throwing saltpetre, or otherwise. And so lead melted with silver quickly pervades and liquifies the silver in a much less heat than is required to melt the silver alone; but if they be kept in the test till that little substance that reconciled them be wasted or altered, they part again of their own accord. And in like manner the aetherial animal spirit in a man may be a mediator between the common æther, and the muscular juices, to make them mix more freely; and so by sending a little of this spirit into any muscle, though so little as to cause no sensible tension of the muscle by its own force, yet by rendering the juices more sociable to the common external æther, it may cause that æther to pervade the muscle of its own accord in a moment more freely and more copiously than it would otherwise do, and to recede again as freely, so soon as this mediator of sociability is retracted; whence, according to what I said above, will proceed the swelling or shrinking of the muscle, and consequently the animal motion depending thereon.

Thus may therefore the soul, by determining this ætherial animal spirit or wind into this or that nerve, perhaps with as much ease as air is moved in open spaces, cause all the motions we see in animals; for the making which motions strong, it is not necessary that we should suppose the æther within the muscle very much condensed, or rarefied, by this means, but only that its spring is so very great that a little alteration of its density shall cause a great alteration in the pressure. And what is said of muscular motion may be applied to the motion of the heart, only with this difference; that the spirit is not sent thither as into other muscles, but continually generated there by the fermentation of the juices with which its flesh is replenished, and as it is generated, let out by starts into the

brain, through some convenient *ductus*, to perform those motions in other muscles by inspiration, which it did in the heart by its generation. For I see not why the ferment in the heart may not raise as subtile a spirit out of its juices, to cause those motions, as rubbing does out of a glass to cause electric attraction, or burning out of fuel to penetrate glass, as Mr. Boyle has shown\*, and calcine by corrosion metals melted therein.

Hitherto I have been contemplating the nature of æther and ætherial substances by their effects and uses, and now I come to join therewith the consideration of light.

In the fourth place, therefore, I suppose light is neither æther, nor its vibrating motion, but something of a different kind propagated from lucid bodies. They that will may suppose it an aggregate of various peripatetic qualities. Others may suppose it multitudes of unimaginable small and swift corpuscles of various sizes springing from shining bodies at great distances one after another, but yet without any sensible interval of time, and continually urged forward by a principle of motion, which in the beginning accelerates them, till the resistance of the aetherial medium equal the force of that principle, much after the manner that bodies let fall in water are accelerated till the resistance of the water equals the force of gravity. God, who gave animals motion beyond our understanding, is, without doubt, able to implant other principles of motions in bodies which we may understand as little. Some would readily grant this may be a spiritual one; yet a mechanical one might be shown, did not I think it better to pass it by. But they that like not this, may suppose light any other corporeal emanation, or an impulse or motion of any other medium or ætherial spirit diffused through the main body of æther, or what else they imagine proper for this purpose. To avoid dispute, and make this hypothesis general, let every man here take his fancy; only, whatever light be, I would suppose it consists of successive rays differing from one another in contingent circumstances, as bigness, force, or vigour, like as the sands on the shore, the waves of the sea, the faces of men, and all other natural things of the same kind

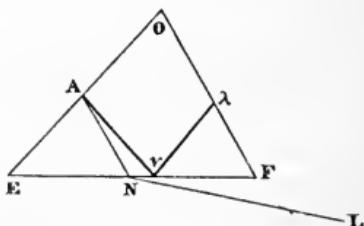
\* Boyle's Essays of the strange subtilty, &c. of effluviums, &c., together with a discovery of the perviousness of glass to ponderable parts of flame.

differ, it being almost impossible for any sort of things to be found without some contingent variety. And further, I would suppose it diverse from the vibrations of the æther, because (besides that were it those vibrations, it ought always to verge copiously in crooked lines into the dark or quiescent medium, destroying all shadows, and to comply readily with any crooked pores or passages as sounds do) I see not how any superficies (as the side of a glass prism on which the rays within are incident at an angle of about forty degrees) can be totally opaque. For the vibrations beating against the refracting confine of the rarer and denser æther must needs make that pliant superficies undulate, and those undulations will stir up and propagate vibrations on the other side. And further, how light, incident on very thin skins or plates of any transparent body, should for many successive thicknesses of the plate in arithmetical progression, be alternately reflected and transmitted, as I find it is, puzzles me as much. For though the arithmetical progression of those thicknesses, which reflect and transmit the rays alternately, argues that it depends upon the number of vibrations between the two superficies of the plate, whether the ray shall be reflected or transmitted, yet I cannot see how the number should vary the case, be it greater or less, whole or broken, unless light be supposed something else than these vibrations. Something indeed I could fancy towards helping the two last difficulties, but nothing which I see not insufficient.

Fifthly, it is to be supposed that light and æther mutually act upon one another, æther in refracting light, and light in warming æther, and that the densest æther acts most strongly. When a ray therefore moves through aether of uneven density, I suppose it most pressed, urged, or acted upon by the medium on that side towards the denser æther, and receives a continual impulse or ply from that side to recede towards the rarer, and so is accelerated if it move that way, or retarded if the contrary. On this ground, if a ray move obliquely through such an unevenly dense medium (that is, obliquely to those imaginary superficies which run through the equally dense parts of the medium, and may be called the refracting superficies), it must be incurved, as it is found to be

by observation in water\*, whose lower parts were made gradually more salt, and so more dense than the upper. And this may be the ground of all refraction and reflexion. For as the rarer air within a small glass pipe, and the denser without, are not distinguished by a mere mathematical superficies, but have air between them at the orifice of the pipe running through all intermediate degrees of density, so I suppose the refracting superficies of æther between unequally dense mediumis to be not a mathematical one, but of some breadth, the æther therein at the orifices of the pores of the solid body being of all intermediate degrees of density between the rarer and denser ætherial mediums; and the refraction I conceive to proceed from the continual incurvation of the ray all the while it is passing the physical superficies. Now if the motion of the ray be supposed in this passage to be increased or diminished in a certain proportion, according to the difference of the densities of the ætherial mediums, and the addition or detraction of the motion be reckoned in the perpendicular from the refracting superficies, as it ought to be, the sines of incidence and refraction will be proportional according to what Descartes has demonstrated.

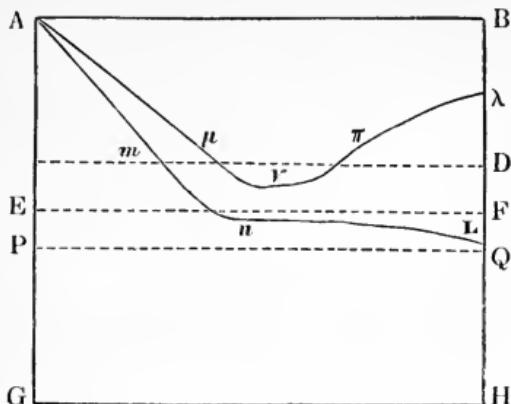
The ray therefore in passing out of the rarer medium into the denser, inclines continually more and more towards parallelism with the refracting superficies; and if the differing densities of the mediums be not so great, nor the incidence of the ray so oblique as to make it parallel to that superficies before it gets through, then it goes through and is refracted; but if through the aforesaid causes the ray becomes parallel to that superficies before it can get through, then it must turn back and be reflected. Thus, for instance, it may be observed in a triangular glass prism O E F, that the rays A N that tend out of the glass into air, do, by inclining them more and more to the refracting superficies, emerge more and more obliquely till they be infinitely oblique, that is, in a manner parallel to the su-



\* Mr. Hook's Micrographia where he speaks of the inflexion of rays.

perficies, which happens when the angle of incidence is about  $40^\circ$ ; and then if they be a little more inclined, are all reflected, as at  $A \nu \lambda$ , becoming, I suppose, parallel to the superficies before they can get through it.

Let  $A B C D$  represent the rarer medium,  $E F H G$  the



denser,  $C D F E$  the space between them or refracting physical superficies, in which the aether is of all intermediate degrees of density, from the rarest aether at  $C D$  to the densest at  $E F$ ;  $A m n L$  a ray,  $A m$  its incident part,  $m n$  its incurvation by the refracting superficies, and  $n L$  its emergent part. Now if the ray  $A m$  be so much incurved as to become at its emergence  $n$ , as nearly as may be, parallel to  $C D$ , it is plain that if that ray had been incident a little more obliquely, it must have become parallel to  $C D$  before it had arrived at  $E F$ , the further side of the refracting superficies, and so could have got no nearer to  $E F$ , but must have turned back by further incurvation, and been reflected as it is represented at  $A \mu \nu \lambda$ : and the like would have happened if the density of the aether had further increased from  $E F$  to  $P Q$ , so that  $P Q H G$  might be a denser medium than  $E F H G$  was supposed; for then the ray in passing from  $m$  to  $n$ , being so much incurved as at  $n$  to become parallel to  $C D$  or  $P Q$ , it's impossible it should ever get nearer to  $P Q$ , but must at  $n$  begin by further incurvation to turn back, and so be reflected. And because if a refracted ray (as  $n L$ ) be made incident, the incident ( $A m$ ) shall become the refracted; and therefore if the ray  $A \mu \nu$ , after it is arrived at  $\nu$ , where I suppose it parallel to the

refracting superficies, should be reflected perpendicularly back, it would return back in the line of incidence  $\nu \mu A$ ; therefore going forward, it must go forward in such another line  $\nu \pi \lambda$ , both cases being alike, and so be reflected at an angle equal to that of incidence.

This may be the cause and manner of reflexion, when light tends from the rarer towards the denser æther; but to know how it should be reflected when it tends from the denser towards the rarer, you are farther to consider, how fluids near their superficies are less pliant and yielding than in their more inward parts, and if formed into thin plates or shells, they become much more stiff and tenacious than otherwise. Thus things which readily fall in water, if let fall upon a bubble of water, they do not easily break through it, but are apt to slide down by the sides of it, if they be not too big and heavy. So if two well-polished convex glasses, ground on very large spheres, be laid one upon the other, the air between them easily recedes till they almost touch, but then begins to resist so much that the weight of the upper glass is too little to bring them together, so as to make the black (mentioned in the other papers I send you) appear in the midst of the rings of colours. And if the glasses be plain, though no broader than a two-pence, a man with his whole strength is not able to press all the air out from between them, so as to make them fully touch. You may observe also that insects will walk upon water without wetting their feet, and the water bearing them up; also motes falling upon water will often lie long upon it without being wetted. And so I suppose æther in the confine of two mediums is less pliant and yielding than in other places, and so much the less pliant by how much the mediums differ more in density; so that in passing out of denser æther into rarer, when there remains but a very little of the denser æther to be passed through, a ray finds more than ordinary difficulty to get through, and so great difficulty where the mediums are of a very differing density as to be reflected by incurvation after the manner described above, the parts of æther on the side where they are less pliant and yielding, acting upon the ray much after the manner that they would do were they denser there than on the other side; for the resistance of the medium

ought to have the same effect on the ray from whatsoever cause it arises. And this I suppose may be the cause of the reflexion of quicksilver and other metalline bodies. It must also concur to increase the reflective virtue of the superficies when rays tend out of the rarer medium into the denser; and in that case therefore the reflexion having a double cause ought to be stronger than in the aether, as it is apparently. But in refraction this rigid tenacity or unpliableness of the superficies need not be considered, because so much as the ray is thereby bent in passing to the most tenacious and rigid part of the superficies, so much is it thereby unbent again in passing on from thence through the next parts gradually less tenacious.

Thus may rays be refracted by some superficies and reflected by others, be the medium they tend into denser or rarer. But it remains further to be explained, how rays alike incident on the same superficies (suppose of crystal, glass or water), may be at the same time, some refracted, others reflected; and for explaining this, I suppose that the rays when they impinge on the rigid resisting æthereal superficies, as they are acted upon by it, so they react upon it, and cause vibrations in it, as stones thrown into water do in its surface; and that these vibrations are propagated every way into both the rarer and denser mediums, as the vibrations of air which cause sound are from a stroke, but yet continue strongest where they began, and alternately contract and dilate the æther in that physical superficies. For it's plain by the heat which light produces in bodies that it is able to put their parts in motion, and much more to heat and put in motion the more tender æther; and it's more probable that it communicates motion to the gross parts of bodies by the mediation of æther than immediately; as, for instance, in the inward parts of quicksilver, tin, silver, and other very opake bodies, by generating vibrations that run through them, than by striking the outward parts only without entering the body. The shock of every single ray may generate many thousand vibrations, and by sending them all over the body, move all the parts, and that perhaps with more motion than it could move one single part by an immediate stroke; for the vibrations, by shaking each particle backward and forward, may every time increase its

motion, as a ringer does a bell by often pulling it, and so at length move the particles to a very great degree of agitation, which neither the simple shock of a ray nor any other motion in the æther, besides a vibrating one, could do. Thus in air shut up in a vessel, the motion of its parts caused by heat, how violent soever, is unable to move the bodies hung in it with either a trembling or progressive motion; but if air be put into a vibrating motion by beating a drum or two, it shakes glass windows, the whole body of a man, and other massy things, especially those of a congruous tone; yea, I have observed it manifestly shake under my feet a cellared free-stone floor of a large hall; so as I believe the immediate stroke of five hundred drum-sticks could not have done, unless perhaps quickly succeeding one another at equal intervals of time. Ætherial vibrations are therefore the best means by which such a subtle agent as light can shake the gross particles of solid bodies to heat them. And so supposing that light impinging on a refracting or reflecting ætherial superficies puts it into a vibrating motion, that physical superficies being by the perpetual appulse of rays always kept in a vibrating motion, and the æther therein continually expanded and compressed by turns; if a ray of light impinge upon it while it is much compressed, I suppose it is then too dense and stiff to let the ray pass through, and so reflects it; but the rays that impinge on it at other times, when it is either expanded by the interval of two vibrations, or not too much compressed and condensed, go through, and are refracted.

These may be the causes of refractions and reflexions in all cases, but for understanding how they come to be so regular, it's further to be considered, that, as in a heap of sand, although the surface be rugged, yet if water be poured on it to fill its pores, the water, so soon as its pores are filled, will evenly overspread the surface, and so much the more evenly as the sand is finer; so, although the surface of all bodies, even the most polished, be rugged, as I conceive, yet when that ruggedness is not too gross and coarse, the refracting ætherial superficies may evenly overspread it. In polishing glass or metal, it is not to be imagined that sand, putty, or other fretting powders should wear the surface so regularly as to make

the front of every particle exactly plane, and all those planes look the same way, as they ought to do in well-polished bodies, were reflexion performed by their parts; but, that those fretting powders should wear the bodies first to a coarse ruggedness, such as is sensible, and then to a finer and finer ruggedness, till it be so fine that the æthereal superficies evenly overspreads it, and so makes the body put on the appearance of a polish, is a very natural and intelligible supposition. So in fluids it is not well to be conceived that the surfaces of their parts should be all plain, and the planes of the superficial parts always kept looking all the same way, notwithstanding that they are in perpetual motion, and yet without these two suppositions, the superficies of fluids could not be so regularly reflexive as they are, were the reflexion done by the parts themselves, and not by an æthereal superficies evenly overspreading the fluid.

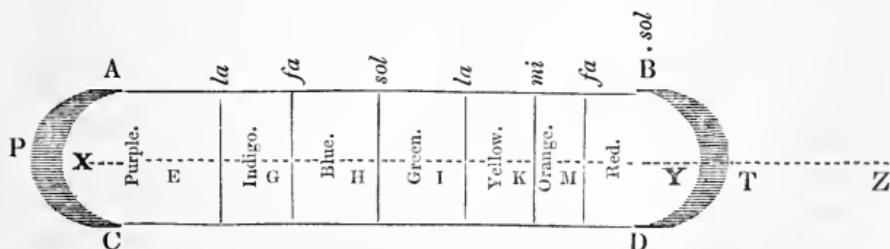
Further, concerning the regular motion of light, it might be suspected whether the various vibrations of the fluid through which it passes may not much disturb it; but that suspicion I suppose will vanish by considering, that if at any time the foremost part of an oblique wave begin to turn it awry, the hindermost part by a contrary action must soon set it straight again.

Lastly, because without doubt there are in every transparent body pores of various sizes, and I said that æther stands at the greatest rarity in the smallest pores, hence the æther in every pore should be of a differing rarity, and so light be refracted in its passage out of every pore into the next, which would cause a great confusion, and spoil the body's transparency; but considering that the æther in all dense bodies is agitated by continual vibrations, and these vibrations cannot be performed without forcing the parts of æther forward and backward from one pore to another by a kind of tremor, so that the æther which one moment is in a great pore, is the next moment forced into a less; and, on the contrary, this must evenly spread the æther into all the pores not exceeding some certain bigness, suppose the breadth of a vibration, and so make it of an even density throughout the transparent body, agreeable to the middle sort of pores. But where the pores

exceed a certain bigness, I suppose the aether suits its density to the bigness of the pore or to the medium within it, and so being of a divers density from the aether that surrounds it, refracts, or reflects light in its superficies, and so makes the body where many such interstices are, appear opake.

Thus much of refraction, reflexion, transparency, and opacity;—and now to explain colours. I suppose that as bodies of various sizes, densities, or tensions, do by percussion or other action, excite sounds of various tones, and consequently vibrations in the air of various bignesses; so, when the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the aether, those rays, whatever they be, as they happen to differ in magnitude, strength, or vigour, excite vibrations of various bignesses; the biggest, strongest, or most potent rays, the largest vibrations, and others shorter according to their bigness, strength, or power; and therefore the ends of the capillamenta of the optic nerve, which front or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations; which vibrations (like those of sound in a trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerves into the sensorium (which light itself cannot do), and there, I suppose, affect the sense with various colours, according to their bigness and mixture: the biggest with the strongest colours, reds and yellows; the least with the weakest, blues and violets; the middle with green, and a confusion of all, with white; much after the manner that in the sense of hearing nature makes use of aërial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed. And further, as the harmony and discord of sounds proceed from the proportions of the aërial vibrations, so may the harmony of some colours, as of a golden and blue, and the discord of others, as of red and blue, proceed from the proportions of the aëtherial. And possibly colour may be distinguished into its principal degrees: red, orange, yellow, green, blue, indigo, and deep violet,—on the same ground that sound within an eighth is graduated into tones. For, some years past, the prismatic colours, being in a well-darkened room, cast perpendicularly

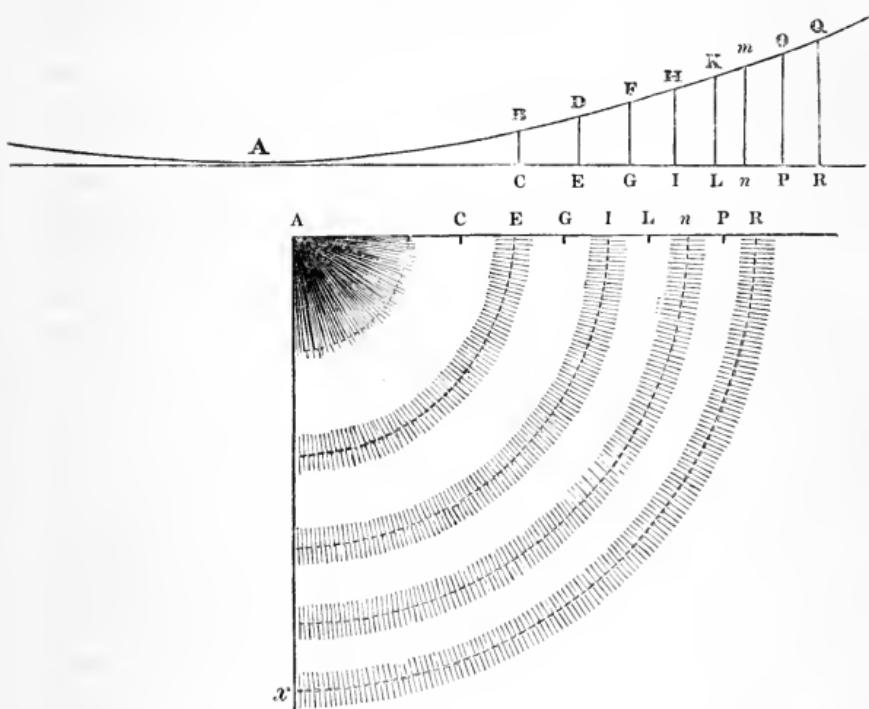
upon a paper about two-and-twenty foot distant from the prism, I desired a friend to draw with a pencil lines across the image or pillar of colours, where every one of the seven aforesigned colours was most full and brisk, and also where he judged the truest confines of them to be, whilst I held the paper so that the said image might fall within a certain compass marked on it. And this I did, partly because my own eyes are not very critical in distinguishing colours, partly because another to whom I had not communicated my thoughts about this matter could have nothing but his eyes to determine his fancy in making those marks. This observation we repeated divers times, both in the same and divers days, to see how the marks on several papers would agree; and comparing the observations, though the just confines of the colours are hard to be assigned, because they passed into one another by insensible gradation, yet the differences of the observations were but little, especially towards the red end; and taking means between those differences that were, the length of the image (reckoned not by the distance of the verges of the semicircular ends, but by the distance of the centres of those semicircles, or length of the straight sides, as it ought to be) was divided in about the same proportion that a string is between the end and the middle to sound the tones in an eighth. You will understand me best by viewing the annexed figure, in which A B and C D represent the straight sides about ten inches long, A P C and B T D the semicircular ends, X and Y the centres of those semicircles, X Z the length of a musical string double to X Y, and divided between X and Y so as to sound the tones expressed at the side (that is X H the half, X G and G I the third part, Y K the fifth part, Y M the eighth part, and G E the ninth part of X Y);



and the intervals between these divisions express the spaces which the colours written there took up, every colour being

most briskly specific in the middle of those spaces. Now for the cause of these and such like colours made by refraction, the biggest or strongest rays must penetrate the refracting superficies more freely and easily than the weaker, and so be less turned awry by it, that is less refracted; which is as much as to say, the rays which make red are least refrangible, those which make blue, or violet, most refrangible, and others otherwise refrangible according to their colour. Whence if the rays which come promiscuously from the sun be refracted by a prism, as in the aforesaid experiment, those of several sorts being variously refracted, must go to several places on an opposite paper or wall, and so parted, exhibit every one their own colours, which they could not do while blended together. And because refraction only severs them, and changes not the bigness or strength of the ray, thence it is, that after they are once well-severed, refraction cannot make any further changes in their colour. On this ground may all the phænomena of refractions be understood. But to explain the colours made by reflexions, I must further suppose, that though light be unimaginably swift, yet the aetherial vibrations excited by a ray move faster than the ray itself, and so overtake and outrun it, one after another. And this I suppose they will think an allowable supposition, who have been inclined to suspect that these vibrations themselves might be light. But to make it the more allowable, it's possible light itself may not be so swift as some are apt to think; for notwithstanding any argument that I know yet to the contrary, it may be an hour or two, if not more, in moving from the sun to us. This celerity of the vibrations therefore supposed, if light be incident on a thin skin or plate of any transparent body, the waves excited by its passage through the first superficies, and taking it one after another till it arrive at the second superficies, will cause it to be there reflected or refracted, accordingly as the condensed or expanded part of the wave overtakes it there. If the plate be of such a thickness that the condensed part of the first wave overtakes the ray at the second superficies, it must be reflected there; if double that thickness, that the following rarefied part of the wave, that is, the space between that and the next wave overtakes it, there it must be transmitted; if triple the thickness, that the condensed part of the second wave

overtake it, there it must be reflected; and so where the plate is five, seven, or nine times that thickness, it must be *reflected* by reason of the third, fourth, or fifth wave overtaking it at the second superficies; but when it is four, six, or eight times that thickness, that the ray may be overtaken there by the dilated interval of those waves, it shall be *transmitted*, and so on; the second superficies being made able or unable to reflect accordingly as it is condensed or expanded by the waves. For instance, let A HQ represent the superficies of a spherically convex glass laid upon a plain glass A IR, and A IR Q H the thin plano-concave plate of air between them, and BC, DE, FG, HI, &c. thicknesses of that plate or distances



of the glasses in the arithmetical progression of the numbers 1, 2, 3, 4, &c., whereof BC is the distance at which the ray is overtaken by the most condensed part of the first wave; I say the rays incident at B, F, K and O ought to be *reflected* at C, G, L and P; and those incident at D, H, M and Q ought to be *transmitted* at E, I, n and R; and this because

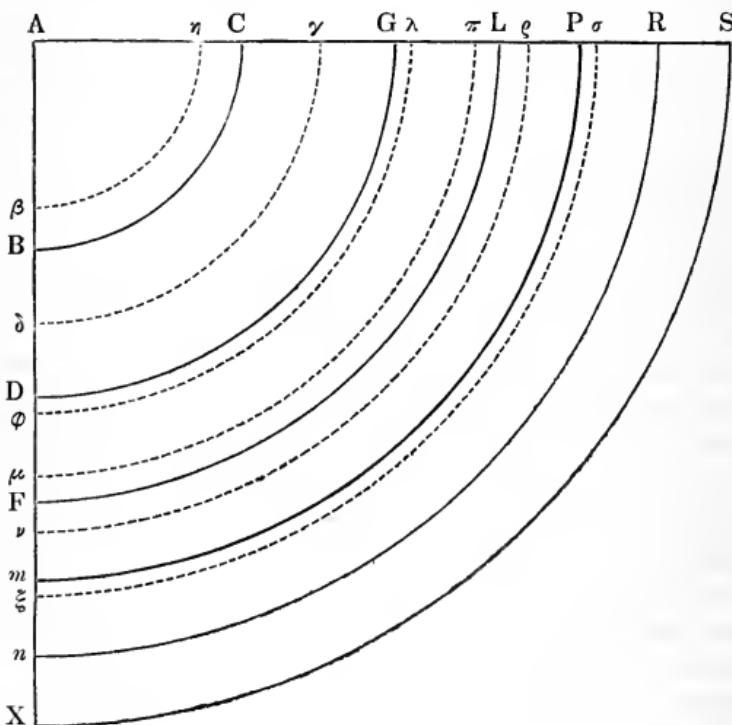
the ray B C arrives at the superficies A C, when it is condensed by the first wave that overtakes it; D E when rarified by the interval of the first and second; F G when condensed by the second wave; H I when rarefied by the interval of the second and third, and so on for an indeterminate number of successions; and at A, the centre, or contact of the glasses, the light must be *transmitted*, because there the aetherial mediums in both glasses are continued as if but one uniform medium. Whence if the glasses in this posture be looked upon, there ought to appear at A, the contact of the glasses, a black spot, and about that many concentric circles of light and darkness, the squares of whose semi-diameters are to sense in arithmetical progression. Yet all the rays without exception ought not to be thus reflected or transmitted: for sometimes a ray may be overtaken at the second superficies by the vibrations raised by another collateral, or immediately succeeding ray; which vibration being as strong, or stronger than its own, may cause it to be reflected or transmitted when its own vibration alone would do the contrary. And hence some little light will be reflected from the black rings, which makes them rather black than totally dark; and some transmitted at the lucid rings, which makes the black rings appearing on the other side of the glasses not so black as they would otherwise be. And so at the central black spot, where the glasses do not absolutely touch, a little light will be reflected, which makes the spot darkest in the middle, and only black at the verges. For thus I have observed it to be, by tying very hard together two glass prisms which were accidentally (one of them at least) a very little convex, and viewing by divers lights this black spot at their contact. If a white paper was placed at a little distance behind a candle, and the candle and paper viewed alternately by reflexion from the spot, the verges of the spot which looked by the light of the paper as black as the middle part, appeared by the stronger light of the candle lucid enough, so as to make the spot seem less than before; but the middle part continued as absolutely black in one case as in the other, some specks and streaks in it only excepted, where I suppose the glasses through some unevenness in the polish did not fully touch. The same I have observed by

viewing the spot by the like reflexion of the sun and clouds alternately.

But to return to the lucid and black rings; those rings ought always to appear after the manner described, were light uniform. And after that manner, when the two contiguous glasses A Q and A R have been illustrated in a dark room by light of any uniform colour made by a prism, I have seen the lucid circles appear to about twenty in number, with many dark ones between them, the colour of the lucid ones being that of the light with which the glasses were illustrated. And if the glasses were held between the eye and the prismatic colours cast on a sheet of white paper, or if any prismatic colour was directly trajected through the glasses to a sheet of paper placed a little way behind, there would appear such other rings of colour and darkness (in the first case between the glasses, in the second on the paper) oppositely corresponding to those which appeared by reflexion. I mean that whereas by reflected light there appeared a black spot in the middle, and then a coloured circle; on the contrary, by transmitted light, there appeared a coloured spot in the middle, then a black circle, and so on; the diameters of the coloured circles made by transmission equalling the diameters of the black ones made by reflexion.

Thus, I say the rings do and ought to appear when made by uniform light, but in compound light it is otherwise. For the rays which exhibit red and yellow, exciting, as I said, larger pulses in the æther than those which make blue and violet, and consequently making bigger circles in a certain proportion, as I have manifestly found they do, by illuminating the glasses successively by the aforesaid colours of the prism in a well-darkened room, without changing the position of my eye or of the glasses; hence the circles made by illustrating the glasses with white light, ought not to appear black and white by turns, as the circles made by illustrating the glasses for instance with red light, appear red and black; but the colours which compound the white light must display themselves by being reflected, the blue and violet nearer to the centre than the red and yellow, whereby every lucid circle must become violet in the inward verge, red in the outward, and of

intermediate colours in the intermediate parts, and be made broader than before, spreading its colours both ways into those spaces which I call the black rings, and which would here appear black, were the red, yellow, blue, and violet, which make the verges of the rings, taken out of the incident white light which illustrates the glasses, and the green only left to make the lucid rings. Suppose C B, G D, L F, P  $m$ , R  $n$ , S X represent quadrants of the circles made in a dark room by the very deepest prismatic *red* alone; and  $\eta\beta$ ,  $\gamma\delta$ ,  $\lambda\phi$ ,  $\pi\mu$ ,  $\rho\nu$ ,  $\sigma\xi$  the quadrants of like circles made also in a dark room, by the very deepest prismatic *violet* alone; and then if the glasses be illuminated by open daylight, in which all sorts of rays are blended, it is manifest that the first lucid ring will be  $\eta\beta$ , BC; the second  $\gamma\delta$ , DG; the third  $\lambda\phi$ , FL; the fourth  $\pi\mu$ ,  $mP$ ; the fifth  $\rho\nu$ ,  $nR$ ; the sixth  $\sigma\xi$ , XS, &c.: in all which the deepest *violet* must be reflected at the inward edges



represented by the pricked lines, where it would be reflected were it alone, and the deepest *red* at the outward edges re-

presented by the black lines, where it would be reflected were it alone, and all intermediate colours at those places in order between these edges at which they would be reflected were they alone; each of them in a dark room parted from all other colours by the refraction of a prism. And because the squares of the semi-diameters of the outward verges  $AC$ ,  $AG$ ,  $AL$ , &c., as also of  $A\eta$ ,  $A\gamma$ ,  $A\lambda$ , &c., the semi-diameters of the inward are in arithmetical progression of the numbers 1, 3, 5, 7, 9, 11, &c.; and the squares of the inward are to the squares of the outward ( $A\eta^9$  to  $AC^9$ ,  $A\gamma^9$  to  $AG^9$ ,  $A\lambda^9$  to  $AL^9$ , &c.) as 9 to 14 (as I have found by measuring them carefully and often, and comparing the observations); therefore the outward *red* verge of the second ring, and inward *violet* one of the third, shall border upon one another (as you may know by computation, and see them represented in the figure), and the like edges of the third and fourth rings shall interfere, and those of the fourth and fifth interfere more, and so on; yea the colours of every ring must spread themselves something more both ways than is here represented, because the quadrantal arcs here described represent not the verges, but the middle of the rings made in a dark room by the extreme violet and red; the *violet* falling on both sides the pricked arches, and *red* on both sides the black line arches. And hence it is, that these rings or circuits of colours succeed one another continually without any intervening black, and that the colours are pure only in the three or four first rings, and then interfering and mixing more and more, dilute one another so much, that after eight or nine rings they are no more to be distinguished, but seem to constitute an even whiteness; whereas when they were made in a dark room, by *one* of the prismatic colours alone, I have, as I said, seen above twenty of them, and without doubt could have seen them to a greater number, had I taken the pains to make the prismatic colour more uncompounded. For by unfolding these rings from one another by certain refractions expressed in the other papers\* I send you, I have even in daylight discovered them to above a hundred, and perhaps they would have appeared innumerable, had the light or colour illustrating the glasses been ab-

\* Obs. 24.

solutely uncompounded, and the pupil of my eye but a mathematical point, so that all the rays which came from the same point of the glass might have gone into my eye at the same obliquity to the glass. What has been hitherto said of these rings is to be understood of their appearance to an unmoved eye; but if you vary the position of the eye, the more obliquely you look on the glass the larger the rings appear. And of this the reason may be, partly that an oblique ray is longer in passing through the first superficies, and so there is more time between the waving forward and backward of that superficies, and consequently a larger wave generated; and partly that the wave in creeping along between the two superficies, may be impeded and retarded by the rigidness of those superficies bounding it at either end, and so not overtake the ray so soon as a wave that moves perpendicularly across.

The bigness of the circles made by every colour and at all obliquities of the eye to the glasses, and the thickness of the air or intervals of the glasses, where each circle is made, you will find expressed in the other papers I send you, where also I have more at large described how much those rings interfere or spread into one another, what colours appear in every ring, where they are most lively, where and how much diluted by mixing with the colours of other rings, and how the contrary colours appear on the back side of the glasses by the transmitted light, the glasses transmitting light of one colour at the same place, where they reflect that of another. Nor need I add anything further of the colours of other thinly plated mediums, as of water between the aforesaid glasses, or formed into bubbles and so encompassed with air, or of glass blown into very thin bubbles at a lamp furnace, &c.; the case being the same in all these, excepting that where the thickness of the plates is not regular, the rings will not be so, that in plates of denser transparent bodies the rings are made at a less thickness of the plate, (the vibrations, I suppose, being shorter in rarer æther than in denser), and that in a denser plate surrounded with a rarer body, the colours are more vivid than in the rarer surrounded with the denser; as for instance, more vivid in a plate of glass surrounded with air, than in a plate of air surrounded with glass; of which the reason is, that the

reflexion of the second superficies, which causes the colours, is, as was said above, stronger in the former case than in the latter; for which reason also the colours are most vivid when the difference of the density of the medium is greatest.

Of the colours of natural bodies also I have said enough in those papers, showing how the various sizes of the transparent particles of which they consist is sufficient to produce them all, those particles reflecting or transmitting this or that sort of rays, according to their thickness, like the aforesaid plates, as if they were fragments thereof. For, I suppose, if a plate of an even thickness, and consequently of an uniform colour, were broken into fragments of the same thickness with the plate, a heap of those fragments would be a powder much of the same colour with the plates. And so, if the parts be of the thickness of the water in the black spot at the top of a bubble described in the seventeenth of the observations I send you, I suppose the body must be black. In the production of which blackness, I suppose, that the particles of that size being disposed to reflect almost no light outward, but to refract it continually in its passage from every part to the next, by this multitude of refractions the rays are kept so long straggling to and fro within the body, till at last almost all impinge on the solid parts of the body, and so are stopped and stifled; those parts having no sufficient elasticity, or other disposition to return nimbly enough the smart shock of the ray back upon it.

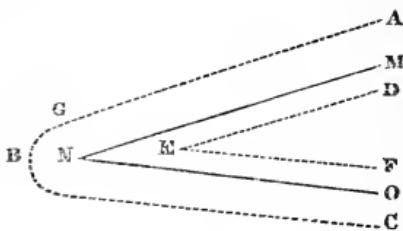
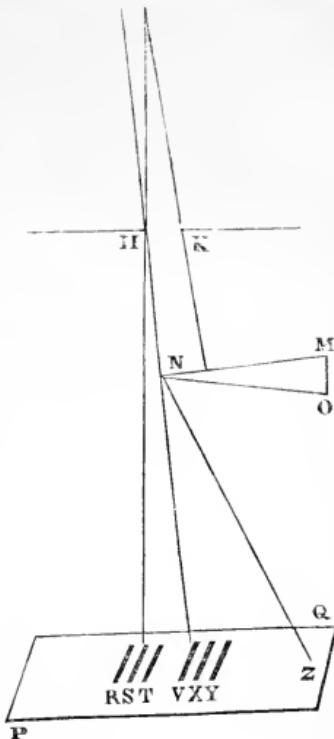
I should here conclude; but that there is another strange phænomenon of colours, which may deserve to be taken notice of. Mr. Hook, you may remember, was speaking of an odd straying of light, caused in its passage near the edge of a razor, knife, or other opake body in a dark room; the rays, which pass very near the edge being thereby made to stray at all angles into the shadow of the knife.

To this Sir William Petty, then president, returned a very pertinent query, whether that straying was in curved lines? and that made me, having heard Mr. Hook some days before compare it to the straying of sound into the quiescent medium, say, that I took it to be only a new kind of refraction, caused perhaps by the external æther's beginning to grow rarer a little

before it came at the opake body, than it was in free spaces, the denser aether without the body, and the rarer within it, being terminated not in a mathematical superficies, but passing into one another through all intermediate degrees of density: whence the rays, that pass so near the body, as to come within that compass where the outward aether begins to grow rarer, must be refracted by the uneven denseness thereof, and bended inwards toward the rarer medium of the body. To this Mr. Hook was then pleased to answer, that though it should be but a new kind of refraction, yet it was a new one. What to make of this unexpected reply I knew not, having no other thoughts, but that a new kind of refraction might be as noble an invention as anything else about light; but it made me afterward, I know not upon what occasion, happen to say, among some that were present to what passed before, that I thought I had seen the experiment before in some Italian author. And the author is Honoratus Faber, in his dialogue *De Lumine*, who had it from Grimaldo; whom I mention, because I am to describe something further out of him, which you will apprehend by the opposite figure. Suppose the sun shine through the little hole H K into a dark room upon the paper PQ, and with a wedge MNO intercept all but a little of that beam, and you will see upon the paper six rows of colours, R, S, T, V, X, Y, and beyond them a very faint light spreading either way, such as rays broken, like HNZ, must make. The author describes it more largely in divers schemes. I have time only to hint the sum of what he says.

Now for the breaking of the ray HNZ, suppose, in the next figure, MNO be the solid wedge, ABC the inward bound of the uniform rarer aether within, between which bounds the aether runs through all the intermediate degrees; and it is manifest, that, if a ray come between B and N, it must in its passage there bend from the denser medium towards C, and that so much the more, by how much it comes nearer N. Further, for the three rows of colours VXY, those may perhaps proceed from the number of vibrations (whether one, two, or three) which overtake the ray in its passage from G, till it be about the midway between G and H, that is at its nearest distance to N, so as to touch the circle described about

N, with that distance; by the last of which vibrations, expanding or contracting the medium there, the ray is licensed to recede again from N, and go on to make the colours; or further bent about N, till the interval of the next wave overtake it, and give it liberty to go from N, very nearly in the line it is then moving, suppose toward Z, to cause the faint light spoken of above. You will understand me a little better, by comparing this with what was said of the colours of thin transparent plates, comparing the greatest distance that the ray goes from GBH towards N, to the thickness of one of those plates. Something too there is in Descartes's explication of the rainbow's colours, which would give further light in this. But I have no time left to insist further upon particulars; nor do I propound this without diffidence, having not made sufficient observation about it.



*Letter from Newton to Oldenburg, dated Jan. 25, 1675-76.*

SIR,

I received both yours, and thank you for your care in disposing those things between me and Mr. Linus. I suppose his friends cannot blame you at all for printing his first letter,

it being written, I believe, for that end, and they never complaining of the printing of that, but of the not printing that which followed, which I take myself to have been, *per accidens*, the occasion of, by refusing to answer him. And though I think I may truly say I was very little concerned about it, yet I must look upon it as the result of your kindness to me that you was unwilling to print it without an answer.

As to the paper of observations which you move in the name of the Society to have printed, I cannot but return them my hearty thanks for the kind acceptance they meet with there, and know not how to deny anything which they desire should be done. Only I think it will be best to suspend the printing of them for a while, because I have some thoughts of writing such another set of observations for determining the manner of the productions of colours by the prism, which if done at all ought to precede that now in your hands, and will do best to be joined with it. But this I cannot do presently by reason of some incumbrances lately put upon me by some friends, and some other business of my own, which at present almost take up my time and thoughts.

The additions that I intended, I think I must, after putting you to so long expectations, disappoint you in; for it puzzles me how to connect them with what I sent you; and if I had those papers, yet I doubt the things I intended will not come in so freely as I thought they might have done. I could send them described without dependence on those papers; but I I fear have already troubled your Society and yourself too much with my scribbling, and so suppose it may do better to defer them till another season. I have therefore at present only sent you two or three alterations, though not of so great moment that I need have stayed you for them; and they are these:—

Where I say that the frame of nature may be nothing but aether condensed by a fermental principle, instead of these words write, that it may be nothing but various contextures of some certain ætherial spirits or vapours condensed, as it were, by precipitation, much after the manner that vapours are condensed into water, or exhalations into grosser substances, though not so easily condensable; and after conden-

sation wrought into various forms, at first by the immediate hand of the Creator, and ever since by the power of nature, who, by virtue of the command, *increase and multiply*, became a complete imitator of the copies set her by the Protoplasm. Thus perhaps may all things be originated from aether, &c.

A little after, when I say the aetherial spirit may be condensed in fermenting or burning bodies, or otherwise inspissated in the pores of the earth to a tender matter, which may be, as it were, the *succus nutritius* of the earth, or primary substance, out of which things generable grow; instead of this you may write, that that spirit may be condensed in fermenting or burning bodies, or otherwise coagulated in the pores of the earth and water into some kind of humid active matter, for the continual uses of nature, adhering to the sides of those pores after the manner that vapours condense on the sides of a vessel.

In the same paragraph there is, I think, a parenthesis, in which I mention volatile salt-petre; pray strike out that parenthesis, lest it should give offence to somebody\*.

Also, where I relate the experiment of little papers made to move variously with a glass rubbed; I would have all that struck out which follows, about trying the experiment of leaf-gold.

Sir, I am interrupted by a visit, and so must in haste break off.

Yours,

January 25, 1675-6.

Is. NEWTON.

*Letter from Newton to Boyle.*

HONOURED SIR,

I have so long deferred to send you my thoughts about the physical qualities we speak of, that did I not esteem myself obliged by promise, I think I should be ashamed to send them at all. The truth is, my notions about things of this kind are so indigested, that I am not well satisfied myself in them; and what I am not satisfied in, I can scarce esteem fit to be com-

\* The "somebody" here meant is Hook, who might have thought these ideas an invasion of his rights to the nitro-aërial theory of combustion.—W. V. H.

municated to others ; especially in natural philosophy, where there is no end of fancying. But because I am indebted to you, and yesterday met with a friend, Mr. Maulverer, who told me he was going to London, and intended to give you the trouble of a visit, I could not forbear to take the opportunity of conveying this to you by him.

It being only an explication of qualities which you desire of me, I shall set down my apprehensions in the form of suppositions as follows. And first, I suppose, that there is diffused through all places an aetherial substance, capable of contraction and dilatation, strongly elastic, and, in a word, much like air in all respects, but far more subtile.

2. I suppose this aether pervades all gross bodies, but yet so as to stand rarer in their pores than in free spaces, and so much the rarer, as their pores are less ; and this I suppose (with others) to be the cause why light incident on those bodies is refracted towards the perpendicular ; why too well-polished metals cohere in a receiver exhausted of air ; why ♀ stands sometimes up to the top of a glass pipe, though much higher than 30 inches ; and one of the main causes why the parts of all bodies cohere ; also the cause of filtration, and of the rising of water in small glass pipes above the surface of the stagnating water they are dipped into ; for I suspect the aether may stand rarer, not only in the insensible pores of bodies, but even in the very sensible cavities of those pipes ; and the same principle may cause menstruum to pervade with violence the pores of the bodies they dissolve, the surrounding aether, as well as the atmosphere, pressing them together.

3. I suppose the rarer aether within bodies, and the denser without them, not to be terminated in a mathematical superficies, but to grow gradually into one another ; the external aether beginning to grow rarer, and the internal to grow denser, at some little distance from the superficies of the body, and running through all intermediate degrees of density in the intermediate spaces ; and this may be the cause why light, in Grimaldo's experiment, passing by the edge of a knife, or other opake body, is turned aside, and as it were refracted, and by that refraction makes several colours. Let ABCD be a dense body whether opake or transparent,

EFGH the outside of the uniform æther, which is within it, IKLM the inside of the uniform æther, which is without it; and conceive the æther, which is between EFGH and IKLM, to run through all intermediate degrees of density between that of the two uniform æthers on either side.

This being supposed, the rays  
of the sun SB, SK, which pass by the edge of this body be-  
tween B and K, ought in their passage through the unequally  
dense æther there, to receive a ply from the denser æther,  
which is on that side towards K, and that the more by how  
much they pass nearer to the body, and thereby to be scat-  
tered through the space PQRST, as by experience they are  
found to be. Now the space between the limits EFGH and  
IKLM, I shall call the space of the æther's graduated rarity.

4. When two bodies moving towards one another come near together, I suppose the æther between them to grow rarer than before, and the spaces of its graduated rarity to extend further from the superficies of the bodies towards one another; and this, by reason that the æther cannot move and play up and down so freely in the straight passage between the bodies, as it could before they came so near together: thus if the space of the æther's graduated rarity reach from the body ABCDFE only to the distance GHL MRS, when no other body is near it, yet may it reach further, as to IK, when another body NOPQ approaches; and as the other body approaches more and more, I suppose the æther between

Fig. 1.

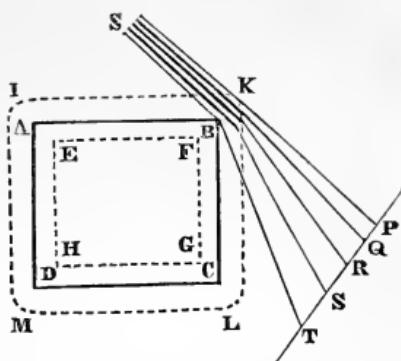
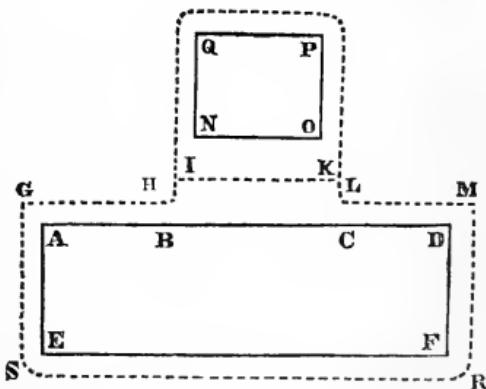


Fig. 2.



them will grow rarer and rarer. These suppositions I have so described, as if I thought the spaces of graduated æther had precise limits, as is expressed at IKLM in the first figure, and GMRS in the second; for thus I thought I could better express myself. But really I do not think they have such precise limits, but rather decay insensibly, and, in so decaying, extend to a much greater distance than can easily be believed or need be supposed.

5. Now from the fourth supposition it follows, that when two bodies approaching one another come so near together as to make the æther between them begin to rarefy, they will begin to have a reluctance from being brought nearer together, and an endeavour to recede from one another; which reluctance and endeavour will increase as they come nearer together, because thereby they cause the interjacent æther to rarefy more and more. But at length, when they come so near together that the excess of pressure of the external æther which surrounds the bodies, above that of the rarefied æther, which is between them, is so great as to overcome the reluctance which the bodies have from being brought together; then will that excess of pressure drive them with violence together, and make them adhere strongly to one another, as was said in the second supposition. For instance, in the second figure, when the bodies E D and N P are so near together that the spaces of the æther's graduated rarity begin to reach to one another, and meet in the line I K, the æther between them will have suffered much rarefaction, which rarefaction requires much force, that is, much pressing of the bodies together; and the endeavour which the æther between them has to return to its former natural state of condensation, will cause the bodies to have an endeavour of receding from one another. But, on the other hand, to counterpoise this endeavour, there will not yet be any excess of density of the æther which surrounds the bodies, above that of the æther which is between them at the line I K. But if the bodies come nearer together, so as to make the æther in the mid-way line I K grow rarer than the surrounding æther, there will arise from the excess of density of the surrounding æther a compression of the bodies towards one another, which, when by the nearer approach of the bodies it

becomes so great as to overcome the aforesaid endeavour the bodies have to recede from one another, they will then go towards one another and adhere together. And, on the contrary, if any power force them asunder to that distance, where the endeavour to recede begins to overcome the endeavour to accede, they will again leap from one another. Now hence I conceive it is chiefly that a fly walks on water without wetting her feet, and consequently without touching the water; that two polished pieces of glass are not without pressure brought to contact, no, not though the one be plain, the other a little convex; that the particles of dust cannot by pressing be made to cohere, as they would do, if they did but fully touch; that the particles of tinging substances and salts dissolved in water do not of their own accord concrete and fall to the bottom, but diffuse themselves all over the liquor, and expand still more if you add more liquor to them. Also, that the particles of vapours, exhalations, and air do stand at a distance from one another, and endeavour to recede as far from one another as the pressure of the incumbent atmosphere will let them; for I conceive the confused mass of vapours, air, and exhalations which we call the atmosphere, to be nothing else but the particles of all sorts of bodies, of which the earth consists, separated from one another, and kept at a distance, by the said principle.

From these principles the actions of menstruum upon bodies may be thus explained: suppose any tinging body, as cochineal or logwood, be put into water; so soon as the water sinks into its pores and wets on all sides any particle which adheres to the body only by the principle in the second superposition, it takes off, or at least much diminishes, the efficacy of that principle to hold the particle to the body, because it makes the æther on all sides the particle to be of a more uniform density than before. And then the particle being shaken off by any little motion, floats in the water, and with many such others makes a tincture; which tincture will be of some lively colour, if the particles be all of the same size and density; otherwise of a dirty one. For the colours of all natural bodies whatever seem to depend on nothing but the various

sizes and densities of their particles, as I think you have seen described by me more at large in another paper. If the particles be very small (as are those of salts, vitriols, and gums), they are transparent; and as they are supposed bigger and bigger, they put on these colours in order, black, white, yellow, red; violet, blue, pale green, yellow, orange, red; purple, blue, green, yellow, orange, red, &c., as it is discerned by the colours, which appear at the several thicknesses of very thin plates of transparent bodies. Whence, to know the causes of the changes of colours, which are often made by the mixtures of several liquors, it is to be considered how the particles of any tincture may have their size or density altered by the infusion of another liquor. When any metal is put into common water, the water cannot enter into its pores, to act on it and dissolve it. Not that water consists of too gross parts for this purpose, but because it is unsociable to metal. For there is a certain secret principle in nature, by which liquors are sociable to some things and unsociable to others; thus water will not mix with oil, but readily with spirit of wine, or with salts; it sinks also into wood, which quicksilver will not; but quicksilver sinks into metals, which, as I said, water will not. So aquafortis dissolves ☽, not ☽; aqua regis ☽, not ☽, &c. But a liquor, which is of itself unsociable to a body, may, by the mixture of a convenient mediator, be made sociable; so molten lead, which alone will not mix with copper, or with regulus of Mars, by the addition of tin is made to mix with either. And water, by the mediation of saline spirits, will mix with metal. Now when any metal is put in water impregnated with such spirits, as into aquafortis, aqua regis, spirit of vitriol, or the like, the particles of the spirits, as they, in floating in the water, strike on the metal, will by their sociableness enter into its pores and gather round its outside particles, and by advantage of the continual tremor the particles of the metal are in, hitch themselves in by degrees between those particles and the body, and loosen them from it; and the water entering into the pores together with the saline spirits, the particles of the metal will be thereby still more loosed, so as by that motion the solution puts them into, to be

easily shaken off, and made to float in the water: the saline particles still encompassing the metallic ones as a coat or shell does a kernel, after the manner expressed in the annexed figure, in which figure I have made the particles round, though they may be cubical, or of any other shape.

Fig. 3.



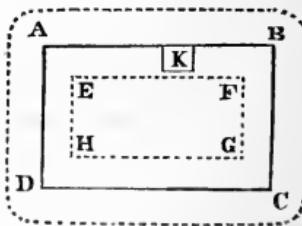
If into a solution of metal thus made be poured a liquor abounding with particles, to which the former saline particles are more sociable than to the particles of the metal (suppose with particles of salt of tartar), then so soon as they strike on one another in the liquor, the saline particles will adhere to those more firmly than to the metalline ones, and by degrees be wrought off from those to enclose these. Suppose A a metalline particle, inclosed with saline ones of spirit of nitre, E a particle of salt of tartar, contiguous to two of the particles of spirit of nitre, b and c; and suppose the particle E is impelled by any motion towards d, so as to roll about the particle c till it touch the particle d, the particle b adhering more firmly to E than to A, will be forced off from A; and by the same means the particle E, as it rolls about A, will tear off the rest of the saline particles from A one after another, till it has got them all, or almost all, about itself. And when the metallic particles are thus divested of the nitrous ones, which, as a mediator between them and the water, held them floating in it, the alcalizate ones, crowding for the room the metallic ones took up before, will press these towards one another, and make them come more easily together: so that by the motion they continually have in the water, they shall be made to strike on one another; and then, by means of the principle in the second supposition, they will cohere and grow into clusters, and fall down by their weight to the bottom, which is called precipitation. In the solution of metals, when a particle is loosing from the body, so soon as it gets to that distance from it, where the principle of receding described in the fourth and fifth supposition begins to overcome the principle of acceding, described in the second supposition, the receding of the particle will be thereby accelerated; so that the particle

Fig. 4.



shall as it were with violence leap from the body, and putting the liquor into a brisk agitation, beget and promote that heat we often find to be caused in solutions of metals. And if any particle happen to leap off thus from the body, before it is surrounded with water, or to leap off with that smartness as to get loose from the water, the water, by the principle in the fourth and fifth suppositions, will be kept off from the particle, and stand round about it, like a spherically hollow arch, not being able to come to a full contact with it any more; and several of these particles afterwards gathering into a cluster, so as by the same principle to stand at a distance from one another, without any water between them, will compose a bubble. Whence I suppose it is, that in brisk solutions there usually happens an ebullition. This is one way of transmuting gross compact substance into aërial ones. Another way is by heat; for as fast as the motion of heat can shake off the particles of water from the surface of it, those particles, by the said principle, will float up and down in the air, at a distance both from one another, and from the particles of air, and make that substance we call vapour. Thus I suppose it is, when the particles of a body are very small (as I suppose those of water are), so that the action of heat alone may be sufficient to shake them asunder. But if the particles be much larger, they then require the greater force of dissolving menstruums to separate them, unless by any means the particles can be first broken into smaller ones. For the most fixed bodies, even gold itself, some have said will become volatile, only by breaking their parts smaller. Thus may the volatility and fixedness of bodies depend on the different sizes of their parts. And on the same difference of size may depend the more or less permanency of aërial substances, in their state of rarefaction. To understand this, let us suppose A B C D to be a large piece of any metal, E F G H the limit of the interior uniform aëther, and K a part of the metal at the superficies A B. If this part or particle K be so little that it reaches

Fig. 5.



not to the limit EF, it is plain that the æther at its centre must be less rare than if the particle were greater; for were it greater, its centre would be further from the superficies AB, that is, in a place where the æther (by supposition) is rarer; the less the particle K therefore, the denser the æther at its centre; because its centre comes nearer to the edge AB, where the æther is denser than within the limit E F G H. And if the particle were divided from the body, and removed to a distance from it, where the æther is still denser, the æther within it must proportionally grow denser. If you consider this, you may apprehend how, by diminishing the particle, the rarity of the æther within it will be diminished, till between the density of the æther without, and the density of the æther within it, there be little difference; that is, till the cause be almost taken away, which should keep this and other such particles at a distance from one another. For that cause explained in the fourth and fifth suppositions, was the excess of density of the external æther above that of the internal. This may be the reason then why the small particles of vapours easily come together, and are reduced back into water, unless the heat, which keeps them in agitation, be so great as to dissipate them as fast as they come together; but the grosser particles of exhalations raised by fermentation keep their aërial form more obstinately, because the æther within them is rarer.

Nor does the size only, but the density of the particles also, conduce to the permanency of aërial substances; for the excess of density of the æther without such particles above that of the æther within them is still greater; which has made me sometimes think that the true permanent air may be of a metallic original; the particles of no substances being more dense than those of metals. This, I think, is also favoured by experience, for I remember I once read in the Philosophical Transactions, how M. Huygens at Paris, found that the air made by dissolving salt of tartar would in two or three days' time condense and fall down again, but the air made by dissolving a metal continued without condensing or relenting in the least. If you consider then, how by the continual fermentations made in the bowels of the earth there are aërial substances raised out of all kinds of bodies, all which together

make the atmosphere, and that of all these the metallic are the most permanent, you will not perhaps think it absurd, that the most permanent part of the atmosphere, which is the true air, should be constituted of these, especially since they are the heaviest of all other, and so must subside to the lower parts of the atmosphere and float upon the surface of the earth, and buoy up the lighter exhalations and vapours to float in greatest plenty above them. Thus, I say, it ought to be with the metallic exhalations raised in the bowels of the earth by the action of acid menstruums, and thus it is with the true permanent air; for this, as in reason it ought to be esteemed the most ponderous part of the atmosphere, because the lowest, so it betrays its ponderosity by making vapours ascend readily in it, by sustaining mists and clouds of snow, and by buoying up gross and ponderous smoke. The air also is the most gross unactive part of the atmosphere, affording living things no nourishment, if deprived of the more tender exhalations and spirits that float in it; and what more unactive and remote from nourishment than metallic bodies?

I shall set down one conjecture more, which came into my mind now as I was writing this letter; it is about the cause of gravity. For this end I will suppose æther to consist of parts differing from one another in *subtilty* by indefinite degrees; that in the pores of bodies there is less of the grosser æther, in proportion to the finer, than in open spaces; and consequently, that in the great body of the earth there is much less of the grosser æther, in proportion to the finer, than in the regions of the air; and that yet the grosser æther in the air affects the upper regions of the earth, and the finer æther in the earth the lower regions of the air, in such a manner, that from the top of the air to the surface of the earth, and again from the surface of the earth to the centre thereof, the æther is insensibly finer and finer. Imagine now any body suspended in the air, or lying on the earth, and the æther being by the hypothesis grosser in the pores, which are in the upper parts of the body, than in those which are in its lower parts, and that grosser æther being less apt to be lodged in those pores than the finer æther below, it will endeavour to get out and give way to the finer æther below, which cannot

be, without the bodies descending to make room above for it to go out into.

From this supposed gradual subtilty of the parts of æther some things above might be further illustrated and made more intelligible; but by what has been said, you will easily discern whether in these conjectures there be any degree of probability, which is all I aim at. For my own part, I have so little fancy to things of this nature, that had not your encouragement moved me to it, I should never, I think, have thus far set pen to paper about them. What is amiss, therefore, I hope you will the more easily pardon in

Your most humble servant and honourer,

Cambridge, Feb. 28, 1678-9.

ISAAC NEWTON.

THE END.

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